Interactive comment on "Modelling fires in the terrestrial carbon balance by incorporating SPITFIRE into the global vegetation model ORCHIDEE – Part 1: Simulating historical global burned area and fire regime" by C. Yue et al.

Anonymous Referee #2
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The paper of Yue et al. documents the development of including the SPITFIRE algorithm into the global vegetation model ORCHIDEE. The paper is clearly written also the graphs are clear and in good quality. The study includes a number of datasets that were not used before in the evaluation of global fire models, e.g. Fire size and fire radiativ power. Although I think it is a great progress to use such datasets for the evaluation it is my main concern about the manuscript that the datasets and the model output are not comparable. For the fire size the datasets are not comparable because the model does not include multiple day burning, moreover the fire duration is limited to 4 hours.

Another factor is that the dataset based on remote sensing misses all the small fires. In Fig. 13 the study even focusses on the 95th quantile of fire size, these largest fires are likely to be burning for longer than the 4 hour limit in the model, therefore it cannot be expected that the model can reproduce this. Yue et al. emphasize that the fire size is very important, but what difference does it make in the model whether the area was burned in one fire or by two fires? Does it make a difference in ORCHIDEE?

For the fire radiative power even the units are different between the satellite data set and the modelled variable, the FRP is per area, the fireline intensity per m. Therefore the fireline intensity in addition to the energy released per area burnt includes how fast the fire was spreading. This may cause differences not only in the absolute values but also in the spatial patterns. I suggest that either the comparison is removed from the paper or a more quivalent fire radiative power is derived from the model output. I think that this should be possible for instance by reverting the procedure of GFAS, where they derive the carbon emissions based on fire raditative power.
[General response] We thank the reviewer for the general positive comments. In response, we have developed an approach to group the fires occurring within consecutive days into "multi-day fire patches", to make the fire size of these fire patches being able to be compared with the observation data which also contains multiday fires. Although fire size does not make a difference in current model simulation (for example, on the combustion completeness of fuels), however it's important to check whether the model could capture the fire size distribution because of at least two reasons (as described in the introduction of our manuscript). First, this will help to diagnose the model error in burned area simulation. Second, the big fires have more severe social and economic consequences and it's important for the model to be able to predict them.

We agree with the reviewer that FRP and simulated fireline intensity is not strictly comparable although their spatial pattern could be similar for the extremely big, fast-spread fires. To avoid any potential misleading, we decide remove this from the manuscript. All the relevant modifications and changes are included in the revised manuscript. To make it easy to follow the revised contents in the manuscript, the heavily modified texts are shown in blue in the revised manuscript.

p.2383, l. 20: maxmimum fire duration is 240 (actually 240.0937) minutes if the equation was not adjusted.
[Response] We used 241minutes as the maximum fire duration time, following the equation (14) on Page 997 of Thonicke et al. (2010).

p. 2384, l. 25: why an additional parameter? was it not possible to increase the necessary fire intensity?
[Response] The original fireline intensity threshold (50kW m^{-1}) is mainly an expert judgement based on what's been described in Pyne (1996). Ideally, the intensity threshold should depend on

fuel load (relating to the amount of energy provided by fire), fuel moisture (relating to the energy needed to heat the adjacent fuel to the ignition temperature), and the fraction of fire-released energy used to heat the adjacent fuel. It's thus difficult to set a single intensity threshold due to the lack of observation data. On the other hand, the use of fuel load to limit the ignition efficiency has been used by other authors (Arora and Boer, 2005; Kloster et al., 2010; Li et al., 2012). The Fig. S2 in our study has also shown that, using the fuel load dependent ignition efficiency has improved the agreement of simulated burned area with the observation for the arid and semi-arid (fuel-limited fire occurrence) regions.

p.2385, l. 8-9: why do you compare the observed "mean" combustion completeness to the "maximum" combustion completeness in the model?

[Response] The combustion completeness (CC) for different fuel types (1h, 10h, 100hr, 1000hr) is simulated as a function of fuel wetness in the model. The fuel wetness is defined as the simulated fuel moisture divided by the prescribed PFT-dependent moisture of extinction. We're calibrating the "maximum" CC as the "mean" value of observed CC, because during the model test we found that the simulated burned area is dominated by very low fuel wetness level (see Figure C1). Given the scheme used to simulate the CC (Fig. 1 in the main text), the mean CC will approach to the maximum CC. So by calibrating the maximum CC as the mean observed, we hope the simulated CC will be realistic. We make the calibration in this way rather than completely drop the CC simulation scheme (and use fixed values of CC), because the scheme used here allows the refinement in the future when more detailed CC observations are available to carefully calibrate the parameters.

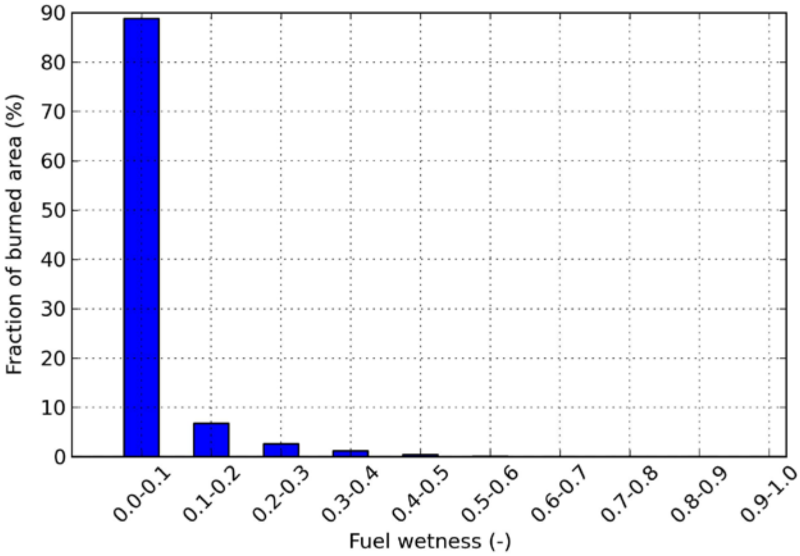

Figure C1. The distribution of global burned area in terms of simulated fuel wetness for 2006. All the 0.5^{o} pixels with fire occurrence across the globe are categorized into ten classes in terms of fuel wetness (shown as the horizontal axis), with the fraction of burned area (in terms of percentage, %) for each fuel wetness class being shown as the vertical axis.

p. 2385, l. 26: what is the reason for the initial spinup without fire? Having the equilibrium of soil pools without the influence of fire should lead to overestimated soil carbon pools and therefore overestimated respiration.

[Response] The intuitive reason to do this initial spinup without fire is to save computation time, as a system without fire would allow fast accumulation in the carbon stocks. All carbon pools (live biomass, aboveground litter and mineral soil) except belowground litter have slightly decreased when the spinup simulation shifted from a fire-free state to a state in which fires are prognostically simulated (Figure C2). The mineral soil carbon stock has been verified to vary within 0.08% during the last 50 years of the spinup. We agree with the reviewer that, even with this decrease in the mineral soil carbon when the fire module was switched on, this carbon stock might still be overestimated given the short time to simulate the fire occurrence before entering the transient simulation. However, we expect the resulting overestimation in respiration would be small because most of these overestimated carbon stocks reside in the passive sub-pool of the mineral soil in the model with a default turnover rate of ~1500 years at 5^{o}C average annual temperature.

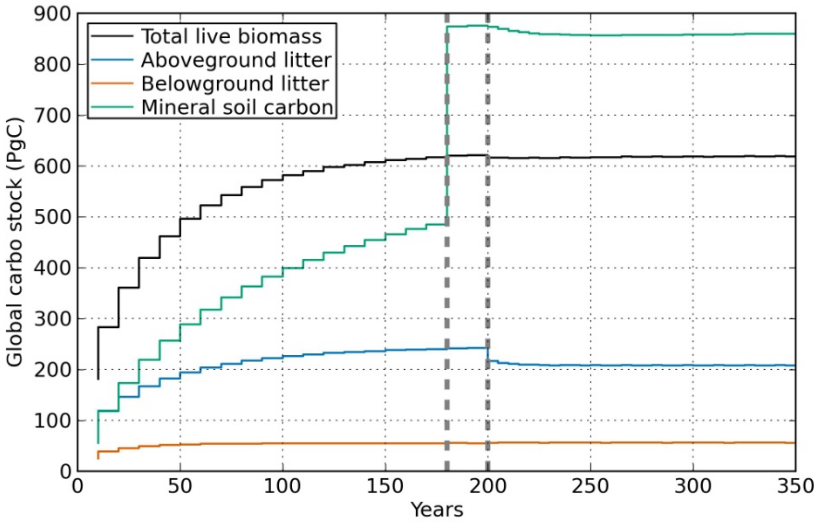

Figure C2 The evolution of global total carbon stock for the live biomass, aboveground litter, belowground litter and mineral soil carbon stock during the spinup simulation. The first vertical dashed gray line indicates that the soil-only processes in ORCHIDEE have been run for 3000 years to speed up the accumulation of mineral soil carbon; and the second vertical dashed gray line indicates the switch-on of the fire module.

p. 2386, l. 15: no land cover change: this may strongly modify the evolution over the 20th century.

[Response] Theoretically, over the long term, the land cover changes (of which the transformation of forest to crops or managed grasslands, and from natural grassland to cropland matter fire the most) will reduce the burned area because the majority of historical land cover change was from forest to managed grassland or croplands; and reduced the forested area available for burning. Over the short term it's rather complex, and might depend on the fire frequency of the land cover before and after transformation. For example, converting a forest with 100-year fire return interval (FRI) by fire into a non-burning cropland will increase the burned area for the year when the forest is burned, but will reduce the burned area for the following 100 years. However, if the cropland is burned each year after the conversion, then the land cover change will increase the burned area. Thus its overall impact on the temporal trend and variation of burned area could be complex.

Very few studies quantified the burned area contributed by land cover change and its net effect on the temporal trend and variability of burned area. Kloster et al., (2010) found that the deforestation and wood harvest for 1850-1990 together reduced the fire carbon emissions in the 1990s by 16% (433 Tg C/year), however the net amount of burned area might be small (assuming a 3000gC m^{-2} of carbon consumption in deforestation fire, the net amount of burned area is 14.4 Mha, or 4% of annual global burned area). We have inserted the following sentence at the end of 3rd paragraph of section 2.3, "This static land cover could affect the model-observation agreement in terms of long-term trend and variation of burned area for regions where land use change fires dominated the burned area.". The regions where model-observation agreement could be affected by the lack of land cover change in the simulation were discussed in section 3.5.

p. 2386: l. 25-29: strange sentence

[Response] We rephrased this part of texts and added Fig. S2 and Fig. S3 for further explanation in the revised manuscript and hope it's more clear, please refer to last paragraph of section 2.3.

p. 2390: l. 10: what happens in grid cells where GFED equals zero? maybe using (GFED+model)/2 could help to be able to include all gridcells in the evaluation (except the ones where both are zero).
[Response] This comparison was done for each GFED region so there is no chance for either the model or GFED burned area to be zero. We add in the revised manuscript that "The evaluation has been done for each GFED3.1 region" (section 2.5.1, first paragraph) to make this clear.

p. 2390, l. 12-14: why do you use the monthly time series for the interannual variation?
[Response] This was originally done to account for the intrinsic seasonality of burned area, but we agree it's better to use the annual burned area time series and now included in the revised manuscript (section 2.5.1).

p. 2390, l.15:eq 3: It took me a while to understand how this is can quantify the similarity in the seasonality. Please explain this is a bit more. for instance explain that the value will be low if the similarity is low, 1 for perfect correlation, what is the value if you compare anticorrelated time series or random time series? what is the advantage compared to a correlation or rank correlation coefficient?
[Response] We argue this indictor (i.e., the overlapping area of the two normalized monthly burned area time series) might be better than the regression slope or correlation coefficient as it retains the physical meaning (i.e., the fraction of burned area in overlapping months against the total annual burned area). In response to the reviewer's comment, a bootstrapping method was used to associate the derived seasonal similarity (S_{season}, see Equation 3 on page 2390 in the discussion paper) with some statistical significance (i.e., the probability that Sseason is from a random distribution of seasonality). This is described in section 2.5.1 and Table 2 in revised manuscript.

p. 2391, l. 28: what are the exact definitions for the categories?
[Response] The exact definitions for the categories are explained in the capital of Fig. 4. Fig. 4 is referred to in the text when the comparison is presented (section 3.1). The definitions are not repeated in the text in order to avoid redundancy.

p.2392, l. 15: Now for the interannual values, the time series is smoothed? please mention this also in the methods section. You could also briefly mention the advantage of a smoothed time series, compared to annual average. In the figure, the seasonality still strongly disctracts from the interannual variability. Where are the correlations mentioned in the methods section? spatial a_nd improvements, possibly also show that the interannual variabitliy is strongly influenced by the african continent?
eq.4: move to methods part. You already have a measure for the seasonality, why are you using another one? Please move to the method section
[Response] The annual series is used in the Fig. 5 in the revised manuscript and correlations are provided at the end of section 3.1. Eq (4) and relevant descriptions are moved to section 2.5.1 in the revised manuscript. The peak fire season, fire season length, and the seasonal similarity are different metrics. The metric in Eq (4) is intended to measure on the global scale the agreement in terms of fire peak month, and can complement the seasonal similarity.

p. 2395, l. 25: mention the model does not include land use change. eq 5. move to methods section.
[Response] The use of static land cover in the simulation is included in section 3.5 (fifth last line) in the revised manuscript. The Eq (5) and the power-law regression analysis is removed because we think the simple comparison already suffice for our purpose (see also the response to the comments by #1 reviewer at Page 11).

p. 2396, l. 17: how big is the minimum fire size in the model and how does this influence the comparison?

[Response] As the power-law regression is removed, the minimum fire size does not matter.

p. 2396, l. 26-27: You state that the simulated fire distribution is skewed towards small fires, big fires are underestimated. The definition for the fire size in the dataset and model is (in my understanding) fundamentally different). The fire size in the datasets used include large fires, that burned over multiple days. In case of the fire model the fire duration is limited to only four hours. but a new fire may start the next day. Therefore this is not surprising. If you can include multiple day burning or estimate from the satellite the size of the fires burned per day (you mention with one dataset that the start and end day are reported) the comparison may therefore be confounded.
[Response] We reconstructed the "multi-day fire patch" to make the simulated fire size being able to be compared with observation. See section 2.5.2 for the method, section 4.2.2 and 4.2.3 for relevant discussions in the revised manuscript. We also use the fire start and end date in the Canadian fire agency data to calculate the fire patch length and compared with the model, see Fig 11c and section 3.6 in the revised manuscript.

p.2397, l. 1-12: This may be strongly influenced by the multiple day burning issue. Does the minimum fire size in the satellite data influence this result?
[Response] The comparison is improved by using the reconstructed "multi-day fire patch" size, the results are updated accordingly, see section 3.6 the third paragraph in the revised manuscript.

p. 2398, l. 1-10: this is the first study that makes use of the FRP global datasets for model evaluation. This is great, but I think the consistency of the comparison can be improved. The units of the two variables are different: FRP is given per m^2, fireline intensity per m. The FRP is the energy of consumed fuel per m-2 burned area. This estimate could be derived as well from the model. The fireline intensity includes the rate of spread as a factor. For the FRP a fast fire that consumes little fuel can have the same FRP value as a slow fire consuming a large amount of fuel. The two datasets are therefore not spatially consistent and it is unclear what the comparison of the two variables with different units can mean. The rate of spread adds spatial patterns to the fireline intensity that may not occur in the FRP datasets. Therefore even when focusing only on the spatial patters the two variables are not comparable. The comparison could be improved by reverting the procedure that is performed when estimating carbon emissions from FRP, this could help to achieve consistency between model and data.
[Response] We agree with the reviewer that these two variables (fireline intensity or FLI, and FRP) are not strictly comparable. The fireline intensity (FLI, in kWm^{-1}) represents the heat transfer per unit length of the fireline, which could be derived as the product of the energy released per square meter (kJm^{-2}) by fire, and the fire spread rate (ms^{-1}) (Byram, 1959). The MODIS FRP measures the radiative energy released in an actively burning fire, by examining the difference in the reflectance of the middle infrared (MIR) band of actively burning pixel and the background pixel.

Because the FRP measures the fire radiative energy on the pixel basis, a pixel with a small fraction being intensively burned will have the same FRP as the pixel with a large fraction being burned but less intensively. This pattern has little relation with the fire spread rate. Besides, the energy loss in fires in forms of conductive and convective energy is not accounted in the FRP (Wooster et al., 2005). However, the FLI accounts for all the energy forms and is closely related with the fire spread rate. Fires with lower fuel consumption (thus less energy release and probably smaller FRP) but with fast spread might have the same FLI as the fires with higher fuel consumption (thus higher energy release and probably larger FRP) but with a low spread rate. Thus their spatial pattern might no be exactly comparable.

Smith and Wooste (2005) reported one approach to derive the FLI from the MODIS FRP. The radiative FLI is derived by dividing the total FRP of the fire front pixels by the fire front length, which is retrieved from the visible imagery. However their derived radiative FLI is one magnitude lower than that from field observations, and their approach does allow large-scale, automatic FLI generation. Before the availability of large-scale FLI observation from satellites, the comparison between simulated and observed FLI will have to be limited on site (or regional) level.

One might think that the total amount of energy (in W or kW) released by fires per square meter simulated by the model (e.g., the product of reaction intensity with fire duration time) could be compared with the MODIS derived fire radiative energy (FRE), which is the FRP being integrated with time. But again, the FRE suffers the same deficiency as FRP that it does not include all the energy forms that a fire releases. The reviewer suggests reverting the GFAS (Kaiser et al., 2012) processes, i.e., if we understand well, probably to use the vegetation-type-dependent conversion factors (Page 533, section 2.3, second paragraph of Kaiser et al., 2012) to adjust the MODIS derived FRE. However, the conversion factors used in GFAS are empirical ratios linking GFAS FRE and GFEDv3.1 emission data. By doing this we are finally in fact comparing the model simulated fuel consumption with GFEDv3.1 data, which is not we want.

So ultimately, the most reliable way is to either have large-scale FLI observations, or compile field-based FLI database which allow direct model-measurement comparison. This could be left for another targeted study and goes beyond the scope of our current one. Finally, to avoid any misleading information by presenting this FLI-FRP comparison, we decide to remove this comparison from our results.

p. 2399, l.11-15: Please mention here that land use was not included in the model simulation.
[Response] The static land cover is included at the end of section 4.1.

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