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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?

Fore 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 equivalent 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.

p.2383, l. 20: maxmimum fire duration is 240 (actually 240.0937) minutes if the equation was not adjusted.

p. 2384, l. 25: why an additional parameter? was it not possible to increase the necessary fire intensity?

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

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.

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

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

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).

p. 2390, l. 12-14: why do you use the monthly time series for the interannual variation?

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?

p. 2391, l. 28: what are the exact definitions for the categories?

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

p. 2395, l. 25: mention the model does not include land use change.

eq 5. move to methods section.

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

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.

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?

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 m2, 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.

p. 2399, l.11-15: Please mention here that land use was not included in the model simulation.

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