Interactive comment on "Modelling the role offires in the terrestrial carbon balance by incorporating SPITFIRE into the global vegetation model ORCHIDEE – Part 2: Carbon emissions and the role of fires in the global carbon balance" by C. Yue et al.

Anonymous Referee #1
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Yue et al. present model results of the impact of fires on the global carbon cycle from simulations with SPITFIRE running inside ORCHIDEE. This paper focuses on carbon emissions from fires and the effect of fires on the terrestrial carbon sink, building off of a companion paper that introduces the model coupling and assesses model performance with regard to fire area burned. The model fire emissions are validated against the GFED although deforestation and agricultural fires are not included in this version of SPITFIRE. This makes the comparison difficult in many respects, in particular the fuel consumption is vastly different between the model and the GFED. The authors do a nice job pointing out this major caveat wherever relevant and make the comparison to GFED natural-fires-only where possible.

The analysis of changes in the terrestrial carbon sink uses the "fire-induced sink reduction" extensively and I find the use of this metric to be an effective way to look at the impacts of fire on the carbon cycle. The authors introduce the idea of a fire "respiration equivalence" which I think is an interesting concept, although I have some suggestions below on how to place this in a clearer context. Overall the paper is nicely presented with appropriate and effective figures, and I list suggestions for improvements below, mostly minor in nature.

[Response] We thank the reviewer for the efforts and the general positive comment on our work. Please find our detailed responses below each of the comments. To facilitate the reading, we marked all modified text in the updated manuscript in red.

General comments:

1. Although this is a companion paper, it would be helpful to the reader who is not familiar with this version of SPITFIRE to include a brief description (one-two paragraphs) of the major aspects of the model somewhere in Section 2. The combustion completeness is discussed in detail but there are a few other things that came to my mind while reading: how does the model treat fire-caused mortality, how are human impacts on fires included (i.e. changes in ignition based on population density but no fire suppression).In addition to these topics, which both come up in subsequent comments here, a basic description of how the model predicts fire events/spread would be helpful.

[Response] Following the reviewer's suggestion we now add two paragraphs in section 2.1, in which brief descriptions of fire physics and the SPITFIRE model and are presented.

2. The definitions of the land-atmosphere carbon fluxes in Section 2.4 could be simplified in my view. The use of both NEP and NBP seems to be superfluous since NEPOFF is the same as NBPOFF, given that the subscript "OFF" implies no fire carbon emission. If only NBP is

used, the SRFIRE equation can be simplified to "SRFIRE=NBPOFF –NBPON" so that it is clear that the SRFIRE is the difference of the same quantity from the two simulations (fire on and fire off). Maybe there is a reason for using both NEP and NBP that I am not seeing, but to me this seems a more clear way to express this quantity.

[Response] We think that the use of NBP_{OFF} rather than NEP_{OFF} in case of fireOFF simulation could potentially reduce the unnecessary complexity. According to the reviewer's suggestion we changed it, and all relevant terms and expressions are now updated in the text.

3. The idea of a "fire respiration equivalence" in the model is interesting, however I think the presentation of this concept could use more explanation. Firstly, Fig. 9d shows that, with the exception of one recent year, SRFIRE is always positive. This should mean that, while increased heterotrophic respiration (RH) in the FireOFF case may compensate for some of the enhanced sink without the fire C flux, it only very rarely compensates for all of the enhanced sink, not only when the fire year was extreme (Pg 9036, Lines 16-19)? If this is the case maybe it would be better to remove references to an "equivalence" and use an alternative description, such as "large-part compensation". Although this does not have the same simplicity of concept even if it may be more appropriate.

Also, some references to the respiration equivalence I think may give the wrong impression about the difference between the real world and a fire/no-fire world comparison. For example, Pg. 9019 Lines 18-19 reads ": : :fires mainly compensate the heterotrophic respiration that would happen if no fires had occurred." I think this gives the impression that in any given year if fires were suddenly turned off then, because of the increase in HR, about the same amount of carbon would be emitted as if fires were still turned on. In this case of the sudden fire switch off, while litter C might increase a bit, and RH with it, it would be a much smaller increase than is seen in a "no-fire" world that has had many years of litter C build-up, such as the FireOFF case in this study. In other words, the RH compensation might only matter relative to a "no-fire" world. So the phrase in the abstract "if no fires had occurred" might be better stated as "in a world without fires". This may seem like a subtle point to make but it is important in my view since readers may get the two ideas confused.

[Response] We agree with the reviewer that the "fire respiration equivalence" might give the impression that emissions are always equal to the excessive respiration in the fireOFF case, which is not true. Therefore we changed this phrase to "fire respiration partial compensation" to indicate the fact that it only partly compensates for the reduction of heterotrophic respiration in fireON case. All relevant expressions are modified in the text.

As pointed out by the reviewer, the expression in Lines 18-19 is indeed a little confusing. We changed it to "in a world without fire" to make the distinction from the case that fires are only turned off few years from a model run with fire.

Specific comments:

Pg 9023, Line 16: The fuel consumption timescale "h" may not be familiar to some readers and could use a definition here.

[Response] We added the following sentence in the 4th paragraph of section 2.1: "The categorization of fuels in terms of magnitude of hours describes the order of magnitude of time required to lose (or gain) 63% of the fuel moisture difference with the equilibrium moisture state under defined atmospheric conditions (Thonicke et al., 2010)."

Pg 9025, Line 23: It might be worth adding here that using constant land cover will mean not only that fires associated with land cover change (deforestation fires) will not be included but also that wildfires will not be affected by changing PFTs.
[Response] We added this sentence in the first paragraph of section 2.3: "The fire-vegetation-climate feedback was not included because the relative fractions of different PFTs remain the same over the simulation period. It means not only that fires associated with land cover change (deforestation fires) are not be included, but also that wildfires are not be affected by changing PFTs."

Pg 9026, Lines 9-10: It is not clear to me how the model as described here is capturing deforestation fires. This should be elaborated on, especially since prior statements indicate that deforestation fires are not included.
[Response] This is now elaborated in the same paragraph where the reviewer raises this comment (2nd paragraph of section 2.3).

Pg 9026, Line 26: Here is where I wondered how population density was taken into account when predicting fire events. A short description of the model in Section 2 would clarify this.
[Response] This is now included in section 2 by adding another two paragraphs describing fire modeling processes.

Pg 9026, Lines 16-26: The criteria for determining model equilibrium is mentioned briefly later on in this section (Pg 9027, Lines 15-17) but it would be helpful to include a description of the criteria used here on Pg 9026 when the spinup is being explained.
[Response] The following sentence is added in the 3rd paragraph of section 2.3, where the spin-up is being explained: "We verify that during last 50 years of this second spin-up, the mineral soil carbon stock varies within 0.1% and no significant trend exists for simulated global carbon balance."

Pg 9028, Lines 22-24: The authors could make more of an effort here to note that peatland fires are not exclusively anthropogenic (especially in N. Hemisphere high latitudes) even though it is convenient to group them this way for the purposes of the study.
[Response] We added the following explanations at the end of the paragraph: " Note that the grouping of different fire types in GFED does not necessarily reflect the exact nature of different fires. For example, peat fires in tropics are mainly due to intentional drainage followed by burning to remove a (bogged) forest (thus anthropogenic, e.g., Marlier et al., 2015), while in northern high-latitude regions peatland fires might be due to drought (thus natural, e.g. Turetsky et al., 2011)."

Pg 9028, Lines 23-24: Another note to include explanation of how some deforestation fires

are captured by the model.

[Response] This is now included in the updated section 2.3 and the readers are thus referred to that section.

Pg 9031, Lines 4-15: This is an excellent point and really adds nicely to the overall analysis. It could be even better with a sentence added that connects the idea laid out in the first sentence of the paragraph (emission variability driven by forest fires, burned area variability driven by grassland/savannah fires) to the difference between the model and GFED.

[Response] The following sentence is added at the end of this paragraph to link the difference again back to the phenomenon pointed out in the first sentence: "because emissions are dominated by litter burning (from grassland, savanna and forest) and are less driven by forest fires that involve large amount of live biomass burning."

Pg 9033, Lines 1-3: The difference in fuel consumption between the model and GFED in tropical grasses/savannah especially is very large, as pointed out in this text and shown in figure 5. Could the authors provide some insight at this point in the text into why the numbers may be so different for these fires (i.e. combustion completeness)?Maybe something as simple as noting that the differences in combustion completeness are driving this discrepancy and they will be discussed in the next section.

[Response] We inserted the following sentence at the end of the paragraph: "The simulated higher fuel consumption in tropical savannas and woodland savannas might be due to a combination of overestimated fuel load and combustion completeness, which is discussed in more detail in section 3.2.4. Further, we acknowledge the fact that ORCHIDEE can have grass and tree PFTs coexisting on the same grid point, but does not describe woody savannas or miombo forests where grass and trees compete locally for water, light and nutrients and could have lower fuel consumptions due to the presence of fire resistant tree species (Hoffmann et al., 2012)".

Pg 9035, Lines 1-3: The van Leeuwen et al. (2014) combustion completeness values might not change the global emissions by much, but maybe they improved the spatial distribution of fuel consumption as compared to the GFED? If so, this could warrant anew figure, or a new panel in Figure 5 showing the difference.

[Response] We are not able to give the spatial distribution of fuel consumption by using combustion completeness (CC) from van Leeuwen et al. (2014) because on Page 9035, Lines 1-3 what we did is a simple adjustment of emissions by using ratios of applied CCs against updated CCs in Leeuwen et al. (2014) rather than a new simulation. We think a new simulation with updated CCs in Leeuwen et al. (2014) could improve the spatial agreement with GFED data in fuel consumption but might not be as much as expected, because fuel load needs also to be accurately calibrated, and simultaneous change in CCs in forest and grassland might partly oppose each other to lower the change, and because van Leeuwen et al. (2014) is in fact not used in GFED data either. However, van Leeuwen et al. (2014) reported for some biomes the CCs for woody litter with different diameter size, which resembles the fuel category used in our model, and this gives a very promising way to include this data in our model in the future. Finally, one has to note that the CCs in GFED data are also derived by scaling the lower and higher boundaries using an estimate for potential evapotranspiration, which is a source of uncertainty. GFED emissions estimates use extensively observed burned area and other datasets (such as tree cover distribution) and a sophisticated scheme to derive emissions, but it is not a fully observation-based data set. Some independent (top-down) approach would rather be needed to constrain the bottom-up emissions estimates.

Section 3.3.1: The figure is really nice, a great concept. Are the data used from the model? If so, would it make more sense to use the full century of model output? This might change the look of the high latitude regions which have longer fire return intervals. I had difficulty understanding parts of this section – Line 12 should read that agricultural harvest PLUS heterotrophic respiration account for 91% off NPP, correct? And I might be missing something but 100% minus RH plus CH (91.0%), minus FE(3.4%) leaves 5.6% for NBP instead of 5.2% as written in the text.

[Response] All data used in Figure 8 are from model output. We have checked the same figure for the 1901-2012 average. The spatial pattern does not change significantly since average annual emission and NPP were used in Figure 8, thus the fire return interval has little influence on the spatial pattern (because mean annual burned fraction is often regarded as the inverse of fire return interval). We put the figure for the 1901-2012 average, and its difference from 2003-2012 average into supplement material as Fig. S6.

We regret the confusion in terms of NPP allocation in component fluxes. As mentioned in section 2.3, there is a remaining positive NBP of 0.19 Pg C yr^{-1} at the end of spin-up and this is subtracted from global simulated NBP when doing analysis on global scale. So if we take this part into account, then the NBP takes up 5.5% of simulated NPP. The percentage of RH plus CH should be 91.1% if the decimal errors in calculation are adjusted. In this way, the different component fluxes add up to 100% of simulated NPP. This is now clarified in the updated texts.

Pg 9036, Line 7: What measure of variability or uncertainty is the plus/minus representing here?

[Response] The standard deviation of SR_{fire} is used.

Pg 9036, Line 14: Is soil organic matter considered available fuel for model fires? This could also be addressed in the brief SPITFIRE description in Section 2.

[Response] We added in section 2 two paragraphs describing briefly the processes of SPITFIRE. The soil organic litter (more often used in boreal regions to refer to accumulated organic matter during intervals between fire disturbances) is treated as "normal litter" in ORCHIDEE and is available for burning.

Pg 9036, Line 19: This is where I thought that discussion of the model treatment of fire-caused mortality of vegetation would be helpful. This role of fires, converting live vegetation C to litter C, should be mentioned here when explaining how fires are analogous to respiration.

[Response] According to the review comments, we briefly described how fire-caused tree mortality is simulated in the model in the updated section 2. There are two basic roles of fires in our model. The first is that fires consume surface litter carbon and part of live biomass carbon and release them into the atmosphere. This role is analogous to heterotrophic respiration and relates to the "fire respiration partial compensation", and is our focus here (in Line 19). This is not to be confused with the second role, that live tree biomass that is killed but not completely combusted by fire will turn into litter. Furthermore, we modified the Line 19 in the revised manuscript to reflect that, the role of fire is "partial" respiration compensation, i.e., fires reduce carbon sink for both low and high fire years, but larger sink reduction occurs for high fire years (as pointed out by the reviewer in previous comments).

Pg 9037, Lines 14-16: This contention would carry more weight if half of the lowest SRfire years were not also after 1970 (after 1980 in this case). Also, precipitation patterns also have a role in determining the high/low years (as the authors note in the following paragraph) and the uncertainty in these fields in the climate data used for atmospheric forcing is high for the first half of the 20th century. I recommend removing this sentence. Also Hartmann et al. (2013) is not listed in the references.
[Response] Considering the uncertainty in the climate data in the first half of the 20th century as pointed out by the reviewer, we removed this sentence and the accompanying reference.

Pg 9038, Lines 12-17: This is stated very nicely. In some ways the SRfire analysis is better without the deforestation fires since it provides a nice wildfire-only baseline to compare anthropogenic impacts against.
[Response] Thanks for the positive comment.

Section 3.3.4: It is a good idea to compare the results of this study to a previous study, Li et al. (2014) in this case. To my mind this discussion can be greatly reduced in scope, maybe even down to just a few sentences. The important parts to note are the big difference in the sink reduction predicted in the two studies, and the major differences in the studies that could lead to this discrepancy.
[Response] We feel like this amount of text is needed for a decent comparison with Li et al. (2014), given that their paper is the only published work addressing a similar issue as ours when we prepared the manuscript.

Table 1: It would be great to have global totals for emissions and burned area included in this table.
[Response] This information is provided as the last row of Table 1.

Figure 6, caption: It was unclear to me what was meant by "based on grid cell area" here. This seems inconsistent with the units given which are per meter squared.
[Response] We have changed the caption to "..., based on the whole grid cell area included both burned and unburned parts." This metric allows easy comparison among different datasets because simply by multiplying the grid cell land area one could get the amount of emissions.

Technical changes:
Pg 9022, Line 11: Delete "issues of what"
[Response] Done.

Pg 9025, Line 7: Change to “A ratio of simulated GPP to MTE-GPP was: : :”
[Response] Done.
Pg 9032, Line 1: “possibly” might be better word choice than “probably” here.
[Response] Done.
Pg 9034, Lines 7-8: This sentence could be stronger if “indicating that the simulated fuel load might be comparable to GFED3.1 data” was deleted.
[Response] We decided not to remove this part, as we cannot fully exclude the error in simulated fuel load. Comparable NPP does not necessarily lead to comparable fuel load because the carbon turnover processes might be different in two models, thus leading to different sizes in litter pool, which is a major fuel source for fire. But as the fuel load data is not provided in Table 4 of van der Werf et al. (2010), we are not able to do the comparison in terms of fuel load as the case of NPP and combustion completeness.

Pg 9037, Lines 11-13: Note that the SRfire numbers used here are global.
[Response] Done.
Pg 9037, Line 16: Change “The SRfire: : :” to “The average SRfire: : :”
[Response] Done.
Pg 9038, Line 23: A couple word choice recommendations – change “limit” to “average”and “accelerated” to “increased”.
[Response] Kelly et al., 2013 as cited used sediment records to examine historical fire frequency, so ‘limit’ corresponds to ‘range’ here which does not give the exact quantitative measurement. Therefore the use of ‘limit’ makes more sense for us. The second recommendation is adopted.
Pg 9039, Line 13: Change “by the two” to “from the two”
[Response] Done.
Pg 9040, Line 13: Change “NPP by fire” to “NPP lost to fire”
[Response] This is an indeed a better way of expressing; change was made.
Pg 9041, Line 17: Is this Table 1 “in” Archibald et al., 2013?
[Response] We changed it to "Table 1 in Archibald et al., 2013"
Table 1 caption: Fix “GEFD”
[Response] Done.
Figure 8 caption: Delete “returned to the atmosphere”
[Response] Done.