Interactive comment on “Modelling the role of fires 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 #2
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[1] Yue and colleagues investigated the spatial and temporal patterns of global fire carbon emissions during 1901-2012 using the ORCHIDEE model. The goal of this work was to evaluate regional characteristics of fire activity (Pyromes, GFED regions) and estimate the net effects of fire activity for global carbon cycling. Model simulations with and without fires considered the potential impacts of fire activity on vegetation carbon storage over a range of time scales. The sequence of analyses stem from simulations of contemporary fire activity, using the GFED3.1 dataset of burned area observations and modeled fire emissions to characterize ORCHIDEE model results. Unfortunately, these comparisons highlight gross disparities between ORCHIDEE results and observations. The inability to reproduce a reasonable pattern of burned area and fire emissions (1997-2010) precludes a more detailed analysis of the century-scale patterns of fire activity and NBP. At these longer time scales, the analysis likely propagates model biases, rather than elucidating carbon cycle dynamics over time scales relevant for carbon cycling.

[Response] We greatly appreciate the efforts of the reviewer who provided us with some very interesting comments and suggestions. Please find our detailed response below each of the original comments. To facilitate the reading, we marked all modified text in the updated manuscript in red.

We explained in section 2.2 and 3.1 that the productivity has been re-calibrated for the model and this led to underestimated burned area compared with prior-calibration. As we focus on fire emissions and their role in carbon cycling for the past century, the precise representation of burned area in the model is considered less important than that of emissions. Figure 5 and Figure 6 shows that the simulated spatial pattern of fire emissions is quite similar with that of the GFED3.1 estimates. In particular, Figure 6 shows the carbon emissions per grid cell area, a metric directly relevant to emissions quantification.

The focus of the current study is the role of fire in terrestrial carbon cycling compared with a model simulation without fire linked to long-term variation of fire carbon emissions. Model evaluation was conducted for a short recent period of 1997-2009 by using GFED3 data. Over the 20th century, we showed in Yue et al. (2014) that the ORCHIDEE-SPITFIRE model can broadly capture the decadal variation of global burned area compared with the historical reconstruction data by Mouillot and Field (2005), the only available global historical reconstruction to our knowledge. In the current study, the simulated vegetation productivity has been re-calibrated and this leads to changes in the magnitude of simulated burned area compared with Yue et al. (2014), but the interannual variability in both burned area and fire emissions remains conserved (correlation coefficient for fire carbon emissions as 0.97 between the two simulations before and after calibration). Given the encouraging model performance when being compared with Mouillot and Field (2005) historical data, we remain reasonably confident on the model results for the past century. Note also that in the FireON simulation, after accounting for fire carbon emissions, simulated NBP (i.e. carbon sink) has a rather reasonably good correlation with the residual land sink (correlation coefficient = 0.59, more details see section 2.3.2). It is rather unlikely that this would have been achieved if simulated historical fire variation had diverged in an unrealistic way from the truth.

However, we do recognize that over the model evaluation period of 1997-2009, despite the quite close total fire emissions to GFED3.1 data, the simulated interannual variability in emissions is underestimated (Figure 3). If this underestimation persists over the historical period, the difference in fire-induced sink reduction (SR_{fire}) between high and low fire years will be underestimated. The estimated mean annual SR_{fire} remains reliable within the limit of our model capacity and simulation framework, because our estimated total fire emissions are close to GFED3.1 data and other modeling studies (see the results from a GFED3.1 burned area forced model simulation in the response to #3 comment).

Finally, some of the limitations in our models are in fact shared by most of the global dynamic vegetation models with fire modules. For example, Li et al. (2013) examined spatial correlation between their simulated grid-cell-area-based fire emissions and those of GFED3.1 data for 1997-2004, and the correlation coefficient ranged from 0.32 to 0.53. The spatial correlation between our fire emissions and GFED3.1 data for 1997-2009 (re-gridded at 2-degree, comparable spatial resolution with Li et al. 2013) is 0.29, slightly lower than Li et al. (2013). If we take a look at other SPITFIRE-based fire models, either no report is made on the comparison on spatial distribution of fire emissions (e.g., Kelley et al., 2014; Pfeiffer et al., 2013; Prentice et al., 2011), or gross disagreements with GFED data exist (e.g., Figure 5 in Lasslop et al., 2014). The same also applies to the temporal pattern of simulated fire emissions. None of the above-mentioned SPITFIRE-based fire modeling studies compared the temporal pattern between simulated fire carbon emissions and GFED3.1 estimates. The model of Yang et al. (2014) simulates fire occurrence and emissions in a different approach from SPITFIRE. They compared annual variability of simulated burned area with GFED3 data for 1997-2007, and the 1998 fire anomaly has been underestimated as well. It seems that the underestimation of 1997-1998 fire emissions anomaly is a common feature of above-mentioned fire models except in Li et al., (2013), where peat burning is included based a prescribed peat distribution map and fixed peatland fire consumption rates. Indeed, a large fraction of 1997-1998 global fire emissions anomaly is contributed by anthropogenic burning in peatland and deforestation fires in tropical Asia and America (Field et al., 2009; Page et al., 2002; van der Werf et al., 2004, 2010). It's unlikely for fire models without a dedicated peatland fire module to be able to capture this anomaly.

Even though our model is far from being 'perfect', the detailed evaluation as presented in Yue et al. (2014), the comparable spatial emission pattern and global total fire emissions with GFED, and the reasonable representation of residual carbon sink estimates, give us confidence and therefore we disagree with the reviewer that some of our model deficiencies preclude the analyses we presented. We acknowledge that model limitation does exist, such as that the underestimation of temporal variability in fire emissions compared with GFED3.1 which is a quasi inventory-based data set. However, we argue that our model also has advantages such as that it could be used to examine long-term historical (as we presented) and future fire activities which inventory-based approach cannot handle simply because of lack of observation data. Based on the detailed model calibrations, we are cautiously confident that our modeling study could provide some meaning insights for the role of fire in the historical carbon cycling.

The likely influence of underestimated interannual variability in fire emissions on the simulated role of fire in the carbon cycling is discussed in section 3.3.3 in the updated manuscript.

[2] Comparisons between ORCHIDEE and GFED3.1 for the spatial and temporal patterns of global fire activity are puzzling (Table 1, and Figures 2-7). Not getting Africa right is a problem, or even wrong for the right reasons, since this is the region with the most consistent patterns of burned area (see Andela& van der Werf, 2014; also van der Werf et al., 2010 and Giglio et al., 2013). Burned area estimates for

Africa are only 27-46% of observations, yet fuel consumption estimates are very high (3-4x). These differences point to significant limitations of the current model setup to reproduce fire activity and fire emissions; NHAF and SHAF are dominated by savanna and woodland fire types, two of the major fire types represented in this version of ORCHIDEE. Australia follows the same pattern («burned area, » fuel consumption). Temporal variability in global burned area and fire emissions is also underrepresented in the model results, with little interannual variability in fire emissions at the regional scale.

[Response] The inclusion of SPITFIRE into the global land surface model ORCHIDEE represents a significant structural change. In Table 1 we compared in a very detailed manner the fire emissions between model simulation and GFED3.1 data and all relevant components to derive fire emissions, in order to identify model errors for future improvement. As we explained in the response to the comment above, burned area here is not the central focus but the comparable total global fire emissions with GFED3.1 is essential to have reliable estimates of NBP and fire-induced sink reduction. Figure 2-4 are mainly for informative purposes. In Figure 5 and 6 the comparisons between simulated fire emissions and GFED3.1 data are reasonable. Figure 7 further complements Table 1 to attribute carbon emission error to burned area and fuel consumption on a latitudinal gradient. So the table and all figures focus on the evaluation of fire emissions, error attribution and implications for future model improvement. We hope that they provide useful information for model calibration in the context of the research objective.

We agree with the reviewer that there is disagreement between model and GFED3.1 data in the simulated burned area and fuel consumption in savanna and woodland fires in Africa and Australia. Comparable fire emission per grid cell area with GFED3.1 estimates can only be made when the errors compensate. We acknowledged and discussed these findings in sections 3.2.3 and 3.2.4. The total fire carbon emissions in Africa and Australia are comparable between model and GFED3.1 because an overestimation in NHAF is partly compensated by underestimation in SHAF. As the total amount of emissions is constrained, we think our model application is valid despite the limitations in reproducing the exact regional pattern of fire activities and emissions. At the same time, we noticed that not getting Africa right is a shared problem of some other models (see Figure 4 in Lasslop et al., 2014; Figure 2 in Prentice et al., 2011; Fig.12 in Pfeiffer et al., 2013). Some disagreements in our results could be linked to a lack of deforestation and agricultural fires and to a too simple parameterization of human ignitions. The disagreements between model simulation and GFED3.1 for land cover types like savanna and woodland could be related with the fundamental approach used to simulate fire processes. For example, given the same fraction of grassland versus tree distribution in a grid cell, evenly-distributed woody vegetation with grass cover between trees would lead to very different fuel characteristics compared to a forest fraction being separated from grasslands. Therefore the fire spread rate would be lower in the first case than the second one. However, this difference cannot be properly represented in the current model setup that represents abstract amount of tree versus grassland fractions. Most DGVMs don’t simulate trees-grass-fires interactions at ecosystem scale (Li et al., 2013; Lasslop et al., 2014; Pfeiffer et al., 2013; Yang et al., 2014). We discussed in detail in Yue et al. (2014) the challenges in simulating mechanistic fire processes in our model but most of these are also shared by other process-based global fire models. To completely solve this issue, more careful and dedicated calibration study needs to be done on regional scale which is currently beyond our scope here. Please also note that we're not using regional specific parameters in the ignition equations, so the first step for improvement might be to have regional parameters before going more deeply in fire processes.

We acknowledged in the response to the #1 comment that the interannual variability in fire emissions is underestimated and the potential influence on our conclusions is now discussed in section 3.3.3. One of the reasons for this underestimation could be related to the different vegetation types that are represented in the model. For example, fires in temperate forest could lead to complete regeneration of new forest patches as a small fraction of a 0.5-degree simulation grid cell. However in the model, this mortality effect is dissipated over the whole grid cell because forest cohorts of different ages after fire are not explicitly represented, thus potentially leading to underestimated damaging effect on trees and subsequent faster forest regrowth. Additional model developments and simulations are needed to confirm the existence of this influence and examine how big it could be on fire activities and emissions.

[3] The validity of the sink reduction (SR) estimates fundamentally depends on getting burned area in the right amounts in the right biomes. Since tropical fires are underestimated, but total emissions are even higher than contemporary estimates from GFED3.1, does this lead to a biased estimate of the SR? Similarly, underestimating interannual variability of fire activity in ORCHIDEE could dampen distinct phases of higher or lower fire activity, with important implications for NBP calculations and contemporary fire risk. Since the model is unable to capture regional and interannual variability in burned area and fire emissions, deeper model interrogation of NBP and SR seems risky, at best (Section 3.3).

[Response] Again, the validity of our study depends on getting the total fire emissions right but not exactly the right burned area. Notably, the total fire emissions for savanna and woodland vegetation in Africa and Australia are in agreement with GFED3.1 data at continental scale. To address precisely the question of the reviewer about the uncertainty of calculated SR_{fire} caused by simulated burned area in the model, we re-run the model for the period of 1997-2009, but with the burned area being externally forced by GFED3 observation data.

When the external burned area data is forced into the model, the SPITFIRE module is operated on a monthly time step, to reconcile the inconsistency of simulated date of burning and reported date of burning. The processes for burned area simulation in the module are simply de-activated, and the input burned area is used directly to calculate the emissions. Emissions from tree live crown scorching and fire-caused tree mortality are not included, because in case of forced burned area, the fire spread rate, fire intensity, tree mortality and relevant variables are not simulated. However, the combustion of live grass and dead fuels on the ground are included, which account for the majority of the total emissions. In terms of the combustion completeness, regional specific values as reported in the GFED3.1 database (as reported in Table 4 in van der Werf et al., 2010 for the 14 regions) are used in the model as constant combustion completeness.

The simulated fire carbon emissions from original ORCHIDEE simulation, ORCHIDEE simulation forced by GFED3 burned area (BA), and fire carbon emissions reported by GFED3.1 data are shown in Figure 1. The agreement in the interannual variability of fire carbon emission between the model and GFED3.1 data improves when the model is forced by GFED3.1 BA data, though not exactly identical. The mean annual emissions for 1997-2009 in the forced BA simulation are 1.8 Pg C yr^{-1}, slightly lower than the 2.0 Pg C yr^{-1} given by GFED3.1 data and the 2.1 Pg C yr^{-1}given by the original simulation. As simulated BA-forced annual emissions deviate from the original simulation, the annual time series of SR_{fire} in the forced simulation also differs from the original simulation. The mean SR_{fire} over 1997-2009 given by the forced simulation is 0.39 Pg C yr^{-1}, slightly lower than the 0.36 Pg C yr^{-1} given by the original simulation. The two estimates are very close to each other, indicating that there is no large bias on SR_{fire} induced by the systematic underestimation of BA. This has been explained in section 3.3.3 the revised manuscript.

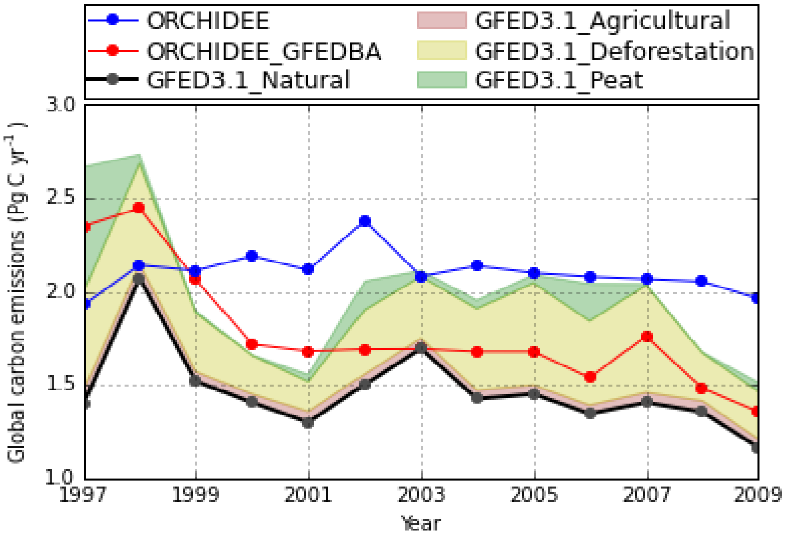

Figure 1 Fire carbon emissions from the original ORCHIDEE simulation (in blue), ORCHIDEE forced by GFED3.1 burned area data (in red), and different GFED3.1 emissions components.

[4] Additional comments on model setup: Static land cover assumption: Estimates of contemporary land cover may be suitable for decadal simulations. However, over century time scales, landscape connectivity and biome distributions can differ substantially. How might land cover differences alter the time series of burned area?

[Response] When we use a static land cover, there are two factors which we do not include in our simulation. The first is natural vegetation change (e.g., biome distribution change as pointed out by the reviewer) induced by environmental changes that are usually simulated by the dynamic vegetation component by DGVM models. The second is the land cover transformation and land characteristics change (such as landscape connectivity as pointed by the reviewer) that are imposed by humans. We showed some results for southern Africa by coupling SPITFIRE with the dynamic vegetation module of ORCHIDEE in Yue et al. (2014) (see discussions in section 4.2.4 and Figure S12 in the supplement in that paper). As the model calibration for fire-vegetation-climate feedbacks is not yet satisfactory (which we believe is the case as well for most fire DGVMs), we chose to not include this component in our simulation as it bring uncertainties that were not simple to interpret and attribute to model structural errors.

Very few studies quantified the burned area contributed by anthropogenic land use and land cover change and their net effect on the temporal trend and variability of burned area. Kloster et al., (2010) estimated that deforestation and wood harvest for 1850-1990 together reduced 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 by GFED3 data). We indicated in section 2.3 that our model captures 92% of GFED3 tropical (20°S–20°N) fire emissions, excluding agricultural and peat fires but including deforestation fires in GFED3. So part of deforestation fire emissions are included in our model but the corresponding burned area is not explicitly represented and separated. Thus we limit our analysis scope more to “naturally occurring vegetation fires” as stated on line 14, Page 9022. We agree with the reviewer that the landscape connectivity as transformed by human activities does influence fires such as fire propagation and potential fire size. However, this fragmentation effect is not included in our model (and almost all other global fire models). To our knowledge no global targeted study tried to examine this issue so far. On the other hand, as the total amount of fire emissions from our simulation is constrained by GFED3.1 data which is build on top of observed burned area (which takes into account the land fragmentation effect), our estimation of global total NBP and SR_{fire} are expected to be credible.

[5] Ignitions: Why was the lightning data cycled, especially for a 5yr period with a strong ENSO event, rather than linking lightning to climate data?
[Response] To our knowledge the LIS/OTD lightning data used in our simulation is the only freely available gridded lightning observation data covering the whole globe to date. We agree with the reviewer that lightning activities are linked with climate state and it should be more reasonable to use temporally variable lightning data rather than cycling the same data. To have varying lightning input, we thus need to calculate lightning flashes from some climate variables. We tried to follow the approach proposed by Pfeiffer et al., (2013) to derive temporally varying lightning data from the convective available potential energy (CPAE) provided by he 20th Century Reanalysis Project (http://portal.nersc.gov/project/20C_Reanalysis/) and applied in our model, however no significant improvement with observed burned area data was found (see more details, please refer to section 4.2.1 Yue et al., 2014 and the accompanying supplement). Krause et al. (2014) explored the impact of changing lightning activities due to climate change on burned area by estimating lightning flashes from convective cloud top height as simulated by coupled MPI earth system model. They found a -0.2% to 3.3% change of global burned area for preindustrial and end-of-21st century RCP85 lightning activities, both being compared with present day burned area. Despite a lot of uncertainties, the overall change in burned area caused by lightning change could be small on the global scale. This is even more the case when considering the dominant human ignition sources in most of the continents, even in some boreal regions (e.g., see Mollicone et al., 2006 on the causes on Russian boreal fires). As most of our analysis relies on a global constrained fire emissions, and taking into account further uncertainties in the reconstructed lightning data, we see little benefit by making the extra efforts to include varying lightning input into our simulation.

[6] Format: The first paragraph of Section 2.2 seems out of place. Comparisons with burned area were done in Yue et al., 2014 and could be referenced, rather than repeated.
[Response] As the vegetation productivity was re-calibrated in our simulation, the simulated burned area changed against to that in Yue et al., (2014). We therefore briefly discussed this change in section 2.2 and also in section 3.1.

[7] Mixture of methods and results in Section 2.3. (e.g., 9026, lines 4-15).
[Response] There is rather no strict clear line between what should belong to the methods and what fits the results. For lines 4-15 on Page 9026, we compared the simulated fire emissions with GFED3.1 data for tropical regions, by separating the emissions into difference sources, notably sources that are explicitly included in our model and those not included. By presenting these in the methods, we think it follows the logic of model introduction regarding what fires are included in our model and not included. We also tried to give an idea how much deforestation fire emissions are implicitly included in our model, as our model could capture part of the "climate windows" that regulate deforestation fires. These are supposed to provide a better context to understand the results that follow. Therefore we think that it is appropriate to present these findings in the methods section.

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