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Biogeosciences Discussions

Interactive comment on "Estimating carbon emissions from African wildfires" *by* V. Lehsten et al.

V. Lehsten et al.

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We would like to express our thanks to the two reviewers for their constructive comments on the manuscript. We hope that the changes that we incorporated as a response improved the quality and clarity of the paper. Here we only list the points raised by the reviewers which may be interesting to a general audience, while the complete list of responses is given in the reviewer specific response. The suggested editorial changes are made in the revised version, and typographical and stylistic errors are removed and listed in the author specific comments.

Our answers are in *italics*. Deletions are referred to the page and line number of the submitted manuscript, not of the revised version, new entries are referred to with the section and paragraph in the revised manuscript.

Anonymous Referee 1

This contribution describes a model assessment of carbon emissions due to wildfires in Africa. The approach is to make use of burn scars as observed by remote sensing techniques, combine these with vegetation maps, model the potential fuel density to estimate the carbon allocated herein, and calculate the emissions subsequently. The authors concentrate on describing their procedures and show their emission results to be in a similar range as found in other approaches. The results are interesting and should be communicated.

Apart from some unnecessary missing citations in the references section, there are some general aspects, which warrant consideration, especially as the paper addresses the general audience.

There is an imbalance between Abstract, Results, Discussion, and Conclusions. In the Abstract it seems, that aside of the amount of released carbon, the main conclusion emerged as the amount of annual precipitation is governing the amount of biomass combustion emissions. The Results speak of burned areas, fire seasonality, carbon emissions, inter-annual variations of burned area and NPP, and finally of precipitation, litter, burned area and emissions. In the Discussion these parameters are correctly detailed, while role of the atmospheric CO concentration peak appears rather unexpectedly.

The CO concentration peak was discussed as an independent estimate of the fire seasonality compared to the seasonality of the burned area in L3JRC. However since serious concerns were raised against using inversion-based estimates (see response to reviewer 2) as a measure for fire/burnt area seasonality, we removed that part from the manuscript (page 3109 line 14 ff).

The abstract and parts of the Method section has been shortened (see also response to rev. 2). We have also edited carefully the discussion section to remove unnecessary detail and remove the perceived imbalance.

and the relationship between burned area and precipitation remains vague.

The relationship between burned area and precipitation was fitted with a generalised

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linear model (GLM), resulting in an r-square of 0.51. The value is significant, but clearly a number of additional factors need to be considered beyond climate alone. This is a well known phenomenon, as also discussed e.g., in Archibald et al. (2008). However, the fitted curve as well as the actual data points clearly peak around 1000mm of precipitation. This latter point is of interest since (as discussed in the text later page 3110 line 2 in the original manuscript), this value is quite similar to the peak of burned area verso precipitation found for tropical savannas in Australia by Spessa et al. (2005).

Finally, the Conclusions accentuate the yet to unravel seasonal atmospheric CO concentration peak over Africa. *See above, seasonal atmospheric CO concentration. Removed.*

The abstract splits the total carbon into CO2, CO, CH4, volatile organic compounds, and black carbon. Though this speciation is helpful, it does not appear later on in the text. Only CO briefly comes up in the discussion part and seems to be the most important topic in the conclusion.
We were using the trace gases in a general statement since pyrogenic emissions do generate fluxes of different carbonaceous trace gas species. However we agree that is not necessary to mention this is in the abstract and removed this part (page 3109 line 14 ff).

Were it useful to spell out the correlation between the results of the L3JRC modeling and the Landsat TM data ? In the end here is the basis of the whole work done. How well defined are the assumptions that 60 % of the total area burnt is underestimated by 42 % ?
The validation experiment was very well defined. 14 Landsat scenes covering various land cover types in sub-Saharan Africa were assessed. For each assessment polygon, the majority land cover was determined (tree cover, shrub cover, croplands etc.). These land cover polygons were grouped and the accuracy assessed for

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each (so independent on geographical location). The validation results are described in the paper by Tansey et al. (2008b) and also on the L3JRC web page (www.tem.jrc.it/Disturbance_by_fire/products/burnt_areas/GlobalBurntAreas2000–2007.htm). This is indicated in the text. It is the first time that a "corrected" burned area estimate has been used. As land cover and burned area within the Landsat scenes are well described, the authors are confident that around 60% of the burned area is underestimated by around 40%. Of course these numbers have to be taken into the context of the accuracy of the GLC2000 product (Section 2.1).

The unavailability of the Thonicke et al. paper makes it difficult to assess the influence of the modeling sequence LPJ-Guess-SPITFIRE-DGVM on the overall results.
We were also expecting to see the Thonicke (2008) paper being available at this stage. If requested by the editor, the manuscript and model technical documentation can be made available to the reviewers. However since the manuscript is already quite long including the section on the SPITFIRE model (Section 2.2), we prefer not to add a more exhaustive model description. Moreover, by using prescribed burnt area, the focus of this manuscript is on effects of vegetation productivity on fire emissions, with productivity/available litter for combustion calculated from the well published and evaluated LPJ-GUESS (examples: Smith et al., Global Ecology and Biogeography, 2001; Morales et al., Global Change Biology, 2005; Hickler et al., GRL, 2005; Arneth et al., ACP, 2007; Miller et al., Journal of Ecology, 2007, etc). Combustion itself is a function of litter moisture using a number of well-established, published algorithms e.g. Peterson and Ryan (1986); Section 2.2.

Were it possible to show the reader the intermediate steps (output one model/input next model) in more detail ?
There is no simple input / output stream between the models. The fire module SPITFIRE is coded into LPJ-GUESS considering specifically the variable age class distribution simulated by the gap-model features; these age classes are influenced

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by the fire regime. There is a high level of interactions between SPITFIRE and other processes that take place on variable time-steps, e.g. the calculation of mortality, establishment, litter decomposition, PFT-distribution, C-allocation etc.. We emphasise this in the manuscript (last paragraph of the Discussion) to make the reader aware of the strong interaction at different scales of the models.

At present all efforts the authors have made are condensed in one table. And the reader hardly has a chance to assess the resulting data. The authors certainly have made sensitivity studies, whose findings may show constraints and give a feeling for accuracy and precision. The standard deviation given in the last column of Table 1 says only something of the variation year to year, but not about the certainly given spread of results of data within one year.

With PJ-GUESS-SPITFIRE we simulated a repeated number of 100 patches for each grid cell to take into account stochastic representation of a number of processes (i.e., related to establishment and mortality; see Smith 2001). The number of patches being burned is derived from the burned area in L3JRC for a given grid cell. Subsequently all patches (which can represent different vegetation stages depending on the last burn) are averaged to obtain vegetation structure, carbon pools and fluxes that are weighed by the amount of area burnt in each grid cell location. Hence to rigorously estimate the spread of the simulated data i.e estimate its statistical distribution, we would have to run the full simulation a hundred times which is computational unfeasible since the standard run already takes a week on 32 processors.

However the sensitivities in the model have been explored: for the SPITFIRE model one of the most sensitive parameter are fuel bulk density and population density Thonike et al. (2008) explores this sensitivity in detail by performing the simulations with different values over the uncertainty range of this values. For LPJ-GUESS, Zaehle et al. (Effects of parameter uncertainties on the modeling of terrestrial biosphere dynamics, Glob. Biogeochem. Cycle, 19(3), 18, 2005) and Wramneby, et al. (Parameter uncertainties in the modelling of vegetation dynamics - Effects on *tree community structure and ecosystem functioning in European forest biomes, Ecol. Model., 216(3-4), 277-29, 2008) did comprehensive analyses of the uncertainties in chief model parameter settings. We added a paragraph (last paragraph of the Discussion section) discussing this issue, highlighting key parameters and refer to the mentioned work to enable the reader to get a feeling for the uncertainty within the vegetation model.*

So this basically asks for an error propagation analysis from model to model.
While this in principle would be useful it cannot be obtained in practice. As highlighted above, Spitfire is an integral component of the LPJ-GUESS code. Since fire and vegetation processes are very tightly coupled and interacts at different time-steps of the model and since there is no singular output of one model that gets fed into the other we see no possibility to perform a reasonable error propagation (last paragraph of Discussion section).

In this area is also the question about what do the coefficients of determination given with Figure 6 tell ? Would a probability, as given in a rigorous statistical analysis taking the number of values into account, help ? Is the mean annual precipitation really shown to be a driver of wildfires?
The p-values derived for GLMs (which take the number of available data points into account) test the "difference from zero for each parameter" so basically whether a linear response is more appropriate than the bell shaped curve that we fitted. All p-values for all parameter are well below 10-10. We added a sentence in the text stating this (Section 3.4 fourth paragraph).

For the reader the statement is based on several times the words "not shown".
Two "not shown" statements are mentioned in 3.4, Relationships between precipitation, litter, burned area and emissions. The first "not shown" refers to the GLM where burned area and litter are used as predictor of annual emissions. We could add a

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figure here if required, but this would have to be three dimensional plot which is not easy to interpret. In our view such a plot would not add crucial information since the shape of the fitted curve is similar to a combination of the two curves 6b (precipitation vs emissions) and 6e (litter vs. emissions) we changed this to not plotted to make this more clear. The second "not shown" refers to the justification for why we choose to analyze the leaf litter instead of the different wood litter classes. Adding a graph for each of the 3 different wood litter classes would in our opinion not contribute to the clarity of the manuscript, since these classes have only low influence on the emissions. Two further "not shown" (Section 3.1) refer to a manuscript in preparation that investigates more closely interactions of climate, fire and canopy structure which is not the focus of this current manuscript. Finally the fifth "not shown" statement in section 3.3 refers to emissions from areas classified as Savannas verso Emissions from the whole continent. If required, we can also add this data in detail, but we already enlarged table 1 and do not see the additional value for the clarity of the manuscript.

It is suggested to put comparison to related work the authors discuss in the text into a table.
This is a good idea and we extended table 1 accordingly.

How large are the assumed non-pyrogenic emissions and uptakes in comparison?
We added the values for anthropogenic fossil fuel carbon emissions as a comparison (first section of Discussion). The continental NPP is already discussed (in the fifth section of the Discussion)

Was the September CO maximum given in the Conclusions the main part of the discussion?
No, it was meant to be an independent derived estimate of the seasonality of CO emis-

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sions and thereby fire emissions, which was compared to our seasonality. However, since there were some doubts raised on the precision of the remotely sensed CO flux seasonality, we decided to take this part out completely.

Anonymous Referee 2

This manuscript presents a new method for estimating carbon emissions from wildfires in Africa that uses the L3JRC burned area product and LPJ-SPITFIRE-DGVM. Overall, this is a thorough and interesting analysis of the factors that influence the magnitude and variability of carbon emissions. However, it would benefit from significant shortening. (For instance, the first paragraph of the abstract could be deleted.)
We rewrote the abstract by removing the first part and focusing more on actual results of this study. By removing the comparison with the remotely sensed CO data the paper was also shortened (and as we hope also gained more clarity).

2) Section 2.4: It may be interesting if you can tie these correlations to large-scale phenomena, such as ENSO.
True, several authors have successfully worked on the effects of ENSO on productivity and fire patterns. We do not discuss this in detail in our manuscript since during the investigated time period there was no strong El Nino or La Nina event. As the manuscript is (as the reviewer mentioned) already quite long we believe a further discussion in this light unwarranted. However, we added a paragraph (third last paragraph of the Discussion) in which we briefly raise this issue, referring the interested reader to a number of studies to this effect.

3) Sections 3.2 3.3: I believe that the paper needs a discussion of the seasonal and interannual variation of emissions for specific regions as estimated by their technique. *To our knowledge, all approaches to estimate seasonal and interannual*

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fire-related carbon emissions are based on continental, hemispherical or at large-regional base. We estimated the variation also for savannas to show the stabilizing effect of non-savanna biomes (i.e. the rainforest) on the variability. We added a paragraph in the text (second paragraph of Discussion).

4) p. 3109, line 21: The MOPITT sensor does not detect CO near the surface in general. It best detects CO at around 500 mb. Therefore, CO is generally not detected until it is lofted to the middle troposphere by convection. As most biomass burning occurs in the dry season, in anticipation of monsoonal rains, the CO may remain undetected by MOPITT for weeks until the pollution builds regionally and encounters convection. Exercise caution when using this dataset to evaluate the timing of your estimate. In fact, I suggest that you remove this discussion as it does not add clarity.
We were not aware of the flaws of this technique especially since it has been used in the same way as we do it in several publications. However, we agree that it is not appropriate and it is not required and was only thought to be an external justification of our simulated seasonality. We therefore removed this part completely.

7) Conclusions: The conclusions are weak. For example, I would suggest that you justify your work in the context of possible future climate change, as you hint at in the second paragraph. What do other studies says about possible climate impacts on Africa, for instance? You have done a lot of interesting work, so please take the time to expand the conclusions.
The conclusion has been re-written, not only to point to further works to be expected to be done with this model but also including to studies assessing the projected climate change impacts on Africa (section 5).

Interactive comment on Biogeosciences Discuss., 5, 3091, 2008.