

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

Anonymous Referee #1

Received and published: 6 November 2008

Comments on

Estimating carbon emissions from African wildfires by V. Lehsten, K. J. Tansey, H. Balzter, K. Thonicke, A. Spessa, U. Weber, B. Smith, and A. Arneeth

submitted to Biogeosciences Discussions

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 and the relationship between burned area and precipitation remains vague. Finally, the Conclusions accentuate the yet to unravel seasonal atmospheric CO concentration peak over Africa. Couldn't some reworking streamline the text ?

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.

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 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. Were it possible to show the reader the intermediate steps (output one model/input next model) in more detail ? 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

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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. So this basically asks for an error propagation analysis from model to model. 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 ? For the reader the statement is based on several times the words 'not shown'.

It is suggested to put comparison to related work the authors discuss in the text into a table.

The authors speak of total carbon emissions. Would pyrogenic carbon emissions be helpful ? How large are the assumed non-pyrogenic emissions and uptakes in comparison?

Why was Andreae and Merlet (2001) cited for CO emissions derived from satellite remote sensing ?

Was the September CO maximum given in the Conclusions the main part of the discussion ?

Minor remarks: Please spell the names of the participating institutions correctly.

page 3099, line 3: are that mass fractions ? page 3100, line 15: Miller et al. is missing page 3101, line 13: Kalney is missing page 3101, line 22: 0.2% significant ? page 3106, line 10: Korintzi is missing, Korontzi ? page 3106, line 13: Korontzi is missing page 3106, line 22: Kalaney is missing page 3108, line 15: DM, presumably dry matter ? page 3110, line 10: (2007) : something missing ? page 3110, line 23: Sankaran et al.