Biogeosciences Discuss., 5, S2740–S2743, 2009
www.biogeosciences-discuss.net/5/S2740/2009/

Biogeosciences Discussions

BGD

5, S2740–S2743, 2009

Interactive Comment

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

V. Lehsten et al.

Received and published: 14 January 2009

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.

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).

Specific Comments

1) Section 2.3: For the sake of brevity, Fig. 1 could be removed as the concept illustrated in it can be stated simply in text.

We would prefer to keep figure 1 because in several oral presentations related to the work the senior author realized that this very important point was not easy to follow. Many readers will not be familiar with forest gap-models and the conceptual figure 1 indicates how the realistic representation of forest growth in gap models can also help to derive a more realistic effect of burning on vegetation, and hence pyrogenic fluxes. This is especially important since other dynamic vegetation models have more simplistic representation of space and reader familiar with these models (and thereby expecting different representation of space) may easily be lost at this early point in the manuscript.

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*

BGD

5, S2740–S2743, 2009

Interactive Comment

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.

5) p. 3099, line 2: This is not a complete sentence.
We changed this to be complete (third paragraph of 2.2).

6) Section 3.4: This analysis is interesting, but you should make the reader understand that precipitation, leaf litter, and burned area are oftentimes non-independent variables. Therefore, your coefficients of determination may be biased by cross-correlation.
Yes this is certainly the case and we added a paragraph where we made that clear to the reader (fourth paragraph of 3.4).

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.