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Biogeosciences Discussions

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5, S2696–S2709, 2009

Interactive Comment

Interactive comment on "Estimates of fire emissions from an active deforestation region in the southern Amazon based on satellite data and biogeochemical modelling" *by* G. R. van der Werf et al.

G. R. van der Werf et al.

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We thank the reviewers for their thorough reviews and thoughtful and constructive comments. We have addressed each of the issues raised by the reviewers, providing specific information regarding changes and improvements to the manuscript in our response below. In several cases, the reviewers asked for more explanation of methods or datasets used in our fire emissions modeling. In response, we have expanded several sections and included a new figure to clarify how input datasets were combined in our approach. Because of our heavy use of methods and data described elsewhere in the literature, we rely on references to previously published work to avoid making the paper overly long. Our reply is shown below.

Referee 1

The emission estimates from tropical forest fires are highly uncertain. This paper presents a meaningful work for the assessment of tropical deforestation fires. The authors developed an emission model at relatively high spatial and temporal resolutions, based on Morton et al. who merged the active fire information with land cover change estimates for deforestation. The work conducted in this paper could substantially contribute to improve the estimation of the fire emissions. As described by the authors, processes not included in this stage (e.g., logging) could be a next step. However, the effect of such processes on the fire emission estimates is not sufficiently discussed in the text. The major concern is that their estimates of deforestation fire emissions might be affected by the logging and understory fires significantly. If the authors provide more detail information on their methods and interpretation of the results, this paper could be improved.

specific comments

1 Introduction You described the bookkeeping methods and inverse modeling in the 3rd paragraph, and then, jumped to the introduction of your model in the 4th paragraph, although you mentioned that current estimates are only available at coarse spatial resolution with large uncertainty in the abstract. Various methods used to estimate the terrestrial carbon fluxes have been compared and discussed in House et al. (2003) and Ito et al. (2008). These studies have shown that large model differences exist in the Amazon forest. Some discussion with the relevant papers would be appropriate. House, J. I., Prentice, I. C., Ramankutty, N., et al.: Reconciling apparent inconsistencies in estimates of terrestrial CO2 sources and sinks, Tellus, Ser. B, 55, 345-363, 2003. Ito, A., J. E. Penner, M. J. Prather, et al.: Can we reconcile differences in estimates of carbon fluxes from land-use change and forestr y for the 1990s?, Atmos. Chem. Phys., 8, 3291-3310, 2008.

BGD

5, S2696–S2709, 2009

Interactive Comment

Following the reviewer's suggestion, we have 1) added the House et al reference in this sentence (page 3535, line 27): "The large range in deforestation carbon loss estimates may be partly due to different processes that are accounted for in the various studies (Houghton, 2003b; House et al., 2003)" 2) added the following sentence to conclude the paragraph (page 3536, line 11): "Apparent inconsistencies between these different approaches may actually be useful to determine different processes. For example, since CO2 inversions measure the full carbon balance of a region including carbon uptake due to forest regrowth, CO2 fertilization, and nitrogen deposition in intact forest, they generally indicate a smaller source from deforestation regions than inventory methods (Ito et al., 2008). The difference between both methods can be used to determine the carbon sink of the region (House et al., 2003). In addition, CO inversions focusing on fires combined with inventory methods may allow for an assessment of committed fluxes."

2.3.1 Deforestation fires and post-clearing land use The INPE deforestation map is a fundamental data set for this study, but the INPE (2007) reference does not help the reader to understand it. You should summarize the data set and cite a specific paper. Brazilian emission inventor y includes the cerrado in the deforestation.

We have expanded this section, which now reads: "The Brazilian National Institute for Space Research (INPE) has mapped annual deforestation rates since 1988 based on Landsat data under the Monitoramento da Floresta Amazônica Brasileira por Satélite (PRODES) project (Shimabukuro et al., 1998). Since 2000, digital data files for annual deforestation in the Brazilian Amazon maps are produced annually and are have been made available from http://www.obt.inpe.br/prodes. The algorithm used in the PRODES project is based upon changes in a pixel's vegetation fraction, derived from established techniques for sup-pixel spectral mixture analysis (Camera et al., 2006)."

Regarding the final question of the reviewer, please see the reply to the next question:

What is the definition of forest in the INPE deforestation map?

BGD

5, S2696–S2709, 2009

The forest/non-forest (Cerrado) boundary was established during a national floristic inventory in the 1970s (RADAMBRASIL). The IBGE/RADAMBRASIL vegetation map remains the basis for vegetation classification in Brazil. The INPE deforestation product only considers forest loss within the Amazon forest biome (INPE, 2007), excluding land cover changes in adjacent woodland or savanna regions. For greenhouse gas emissions purposes (Kyoto 1990 baseline), Brazil included all land cover changes within the country. However, we do not reference this scenario (MCT, 2004) as the timeframe and processes differ from those described in our study.

How did you adjust your model to be consistent with that?

Our model is consistent with the INPE PRODES analysis, in that forest losses within the Brazilian Legal Amazon are considered deforestation. Land cover changes (e.g., the conversion of Cerrado to cropland) were mapped using satellite data products within our study area. Vegetation classes were categorized according to a MODIS-based land cover map (Morton et al., in press), and NPP allocation in the DECAF model was adjusted on this basis. This is now mentioned in the caption of the new figure 5 (see response to reviewer 2).

How did you discriminate the deforestation fires from the logging in the satellite-derived map when you converted the PRODES data to 250m resolution? You stated that the logging process was not included, but the reader might take that the logging was included in the deforestation fires. If the selective logging is treated as the deforestation fires, you may overestimate the fire emissions at relatively high spatial and temporal resolutions significantly, although the reductions in forest biomass from selective logging before deforestation might be small on average. It would be worth adding a few sentences to discuss how this assumption might be expected to affect the estimates presented here. Quantitative discussion would be helpful.

Selective logging is in general not included in INPE deforestation estimates, as the changes are most often too small to be detected by PRODES. One could argue that

BGD

5, S2696–S2709, 2009

Interactive Comment

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Interactive Discussion

Discussion Paper

selective logging lowers the average biomass loads in the region, leaving less biomass available for combustion. This should be partly captured by our methodology because logging precedes conversion, and should lower NDVI values, and thus also estimated biomass in DECAF. Asner et al. (2005) noted that selective logging led to $<$10 Mg C / ha of wood removal, within the uncertainty range for our modeled biomass loads. To address this issue in the text, we have extended the uncertainty section (page 3552, line 20): "and our biomass and fuel loads thus are more likely to be conservative than too high, although selective logging and understory fires may lower biomass loads prior to deforestation (Asner et al., 2005; Balch et al., 2008). Since forest degradation likely lowers the NDVI signal, the resulting decrease in biomass loads may be partly covered by our approach."

Where is Sect. 2.2.5?

We have referred to section 2.5 as section 2.2.5 once. This is now changed

Why did you describe the secondary transition in this section?

This section is titled "2.3.1 Deforestation fires and post-clearing land use" where we describe how different datasets are combined to retrieve the model input data. Secondary transitions are an integral part of this.

2.3.2 Cropland expansion What is the definition of previously-forested areas (INPE, 2007)? How did you identify these areas?

As noted above, the PRODES digital data products use a landcover classification from a national floristic survey (RADAMBRASIL) to identify deforestation of tropical rainforest. Within the historic extent of tropical forest, land cover is classified as forest, recent deforestation, or historic deforestation.

For clarification, pg. 3544 line 4 has been changed to read: "In previously-forested areas ('historic deforestation,' INPE, 2007), the classification approach developed by Morton et al. (2006) was used to identify the conversion of pasture to cropland."

BGD

5, S2696–S2709, 2009

Interactive Comment

What is the definition of non-forest areas (Morton et al., in press)? How did you identify these areas?

The PRODES dataset does not include land cover changes in non-forest land cover types. Therefore, we used a different method to quantify cropland expansion in these areas.

We have changed the description of our approach in non-forest areas (pg. 3544, line 6) to read: "In areas considered non-forest by PRODES (INPE, 2007), including the southern half of Mato Grosso state that is dominated by Cerrado vegetation, a more detailed approach was used to quantify cropland expansion in savanna or woodland regions (Morton et al. 2008)."

In addition, a new figure (described below) and the associated caption also clarify how land cover changes were identified in forest, pasture, and Cerrado regions, based on the combination of data from PRODES (INPE, 2007) and satellite-based land cover classifications (Morton et al., 2006; Morton et al., in press). Please see the response to reviewer # 2 for more information.

2.3.2 Management fires in pasture or cerrado: Which land cover map was used to classify the land into the pasture or cerrado? 2.5 Land cover: How did you summarize the land cover into 6 land cover classes? What is the definition of each class?

For clarification, we have added the figure 1 reference on pg. 3544 line 15: "fires for pasture management or in cerrado savanna-woodland vegetation (Figure 1) that did not result in a conversion to cropland were identified using a 500m MODIS burned area product (Giglio et al. 2009)"

A full description of the land cover classification shown in Figure 1 can be found in Morton et al. (in press). We feel it would be difficult to provide a succinct summary of the field data and remote sensing methods used to develop the land cover classification without detracting from the main purpose of the article. Therefore, we hope that interested parties will look to these other studies for further information.

3 Results and 4 Discussion Although the Landsat-based estimates of deforested area may be the best available data, you have made assumptions in the areas affected by fires. You should have the subsections for the areas affected by fires in results and discussion. It is interesting to compare the areas affected by fires for deforestation and conversion of pasture or Cerrado to cropland with the MODIS burned area product used for maintenance fires and other products.

We partly agree with the reviewer but had organized our paper so that the issues raised by the reviewer are covered in the uncertainty section (4.4, first paragraph) to keep the paper easier to read. We have added a sentence stating that also the deforestation rates are not without uncertainty in this section (page 3552, line 12: ", although not without uncertainty.".

Regarding comparison with the MODIS burned area product: this product performs relatively poor in our study region (Roy et al., 2008). This is one of the main reasons for our complicated route to estimate deforested area and burned area, and we therefore don't see added value of comparing our method to a product that was not optimized for deforestation regions.

Comparison of fuel consumption with measurements (e.g., Balch et al., 2008) would be appreciated.

The results from Balch et al. (2008) were based on understory fires, which are not included in our study and thus not useful for comparison. We were not able to find literature data on fuel consumption in this region, and had therefore combined our own field expertise with three different scenarios on fuel consumption to provide an uncertainty assessment on this poorly quantified parameter. We devoted most of section 2.4 (Modeling fire in DECAF) to this issue where we also mention the range of estimates from studies in the Amazon and have made no further additions.

BGD

5, S2696–S2709, 2009

4.4 Uncertainties Active fire products at 1 km spatial resolution may include some understory fires, but the discussions on the understory fires are not given in this paper, although the paper by Balch et al. (2008) is cited to compare the biomass. The understory fires might be used to determine the transition type (early trajectory) and duration of the transition. The understory fires might have already consumed par t of the woody debris in the tree-covered areas. The understory fires in closed-canopy tropical forests might cause substantial carbon emissions. It would be worth adding a paragraph to discuss how these might be expected to change the estimates presented here. Quantitative discussion would be helpful.

The reviewer raises an important point. Understory fires are not covered by our approach, but -just like selective logging as mentioned by the reviewer above- would lower the biomass available for combustion. This is now more clearly in the uncertainty section: "and our biomass and fuel loads thus are more likely to be conservative than too high, although selective logging and understory fires may lower biomass loads. Since selective logging likely influences the NDVI signal, the resulting decrease in biomass loads may be partly covered by our approach."

The suggestion from the reviewer to use understory fires as an extra metric to understand the trajectory is potentially useful, but we don't know of any way to distinguish these fires from other types of fires. We also want to note that we do not use active fires as a metric for deforestation rates, but merely to understand what types of deforestation are associated with the land cover change.

Referee 2

fires play an important role in the global carbon cycle, and yet the processes that determine fire emissions are complex, local, and crucially involve human drivers. Therefore, fire has so far eluded attempts at accurate modelling on the global scale. This manuscript describes an interesting approach that goes in the direction of tackling this problem. By using simple modelling approaches, but basing their calculations on

BGD

5, S2696–S2709, 2009

Interactive Comment

rather detailed regional data and remote sensing products, the authors have been able to bridge important gaps in scale, that are in principle applicable across the globe. The text is generally well written and comprehensible. As the authors show themselves, the modelling approach is rather simple, which leads to a significant under-estimate in the spatial variability of the fire fluxes. The authors fully document and acknowledge this. In general, I find that the scientific insights this paper provides are somewhat limited, although interesting, but the methodology proposed is promising with some improvements. I would therefore recommend full publication as in BG, with some minor improvements in presentation:

1 - In general, the approach used to account for the different fire activities and possible land use changes could be presented in more detail, while less details is needed for developing carbon model of which only a small par t is really used (the one that leads to the calculation of biomass). I would suggest presenting those in a matrix from (land from - to -), or similar, at least for the land use change. At present, it is difficult to follow through the myriad of cases and exceptions.

We agree with the reviewer that the novel aspects deserve most detail. In the present manuscript, we had devoted only 1.5 pages to the carbon model and provide only the necessary background information and the modifications made. The fire part, on the other hand, covers 5 pages. Although we could provide more detail, we feel this would make the paper even more difficult to read because of the large number of input datasets used. However, we have inserted a new figure following the reviewer's suggestion summarizing the land use change trajectories:

see http://i39.tinypic.com/ic73p3.png for the image, the caption reads:

Figure 5. Land use (LU) change options in DECAF. Pre-clearing LU (see Figure 1) is derived from Morton et al. (in press) and is consistent with the delineation of forest and non-forest areas in PRODES (INPE, 2007). Fire observations are either MODIS burned area (BA) in pasture or Cerrado or PRODES deforestation areas combined

BGD

5, S2696–S2709, 2009

Interactive Comment

with MODIS active fire detections (FD) in forested regions, where x denotes the year that PRODES detected deforestation. Fires x-1 describes the condition where fires were detected in the year prior to deforestation mapping, whereas Fires x+1 indicates that initial MODIS active fire detections in a deforested area occurred in the year after deforestation was detected. The timing and duration of fire activity and post-clearing LU were used to define the transition type for each deforestation event (see Table 3). Fire-related transitions in Cerrado or pasture land cover types are divided between cropland conversion and maintenance fires in DECAF.

2 A figure summarizing all the data sources would be much welcome.

Due to the large variety of datasets used we had inserted figure 3 summarizing how the various different input datasets feed into our model. We refer to the this figure more explicitly in the text now.

Some specific comments:

Page 3539, bottom: Since the NDVI is sensor dependent (as the bands it involves are not the same for each sensor), it is not clear whether the cited methodology to go from NDVI to FAPAR can be applied to MODIS, or has been specifically tuned to the MODIS bands involved. This needs commenting.

Different satellite products in general give different NDVI values. However, the methodology used basically scales the NDVI range so that FPAR ranges from 0 to 0.95 so that maximum NDVI values translate into an FPAR of 0.95.

We have changed the sentence to "NDVI was converted to fAPAR to provide a full range of possible fAPAR values following techniques developed..."

Page 3540, top: epsilon is 0.5, but in what units. Could also mention briefly what the basis is for selecting this value, rather then just citing the CASA model original paper.

We have changed the sentence to: "We set E to 0.5 based on comparison of modeled and measured NPP (van der Werf et al., 2006)"

BGD

5, S2696–S2709, 2009

Interactive Comment

Section 2.2.4, "conversion of pasture or Cerrado to cropland". This sounds as if Cerrado was a xplanato y vegetation cover. Please comment.

This has been changed to "... (labeled as secondary transitions, even though many Cerrado areas are primary vegetation)... "

Page 3542, 2nd para: "Based on phenology...". Please detail how phenology is observed here.

More details are given in the paper cited (Morton et al., 2006). We have chosen not to fully explain all methods and datasets described in the literature to keep the paper concise.

Page 3543, top para: This one would greatly benefit from an schematic diagramme.

This is now covered in the new figure 5

Same page, bottom para: "Sect. 2.2.5". This section seems to be missing.

We referred erroneous to section 2.2.5, which should have been section 2.5. This has been changed

Same para: here, a 2D table with comments would probably help.

This is now covered in the new figure 5

Section 2.5, last sentence: This is because DECAF has epsilon=0.5 constant for all land cover and vegetation types, isn't it? Please mention.

Yes, it partly is. We have added "as none of the model calculations depend on land cover."

Section 3.3 "If we also take soil carbon ...". Didn't you earlier explain that the soil carbon model had not been calibrated yet? Please clarify.

The reviewer is right. We have inserted the sentence "It should be noted, however, that the soil carbon pools are not tuned specifically to this region so these numbers are

BGD

5, S2696–S2709, 2009

Interactive Comment

uncertain".

Section 4.2, 2nd para, beginning: How is the word "related" qualified here? It would be good to see the results of a proper statistical test so that we can better judge this statement. In particular since it relates to an important result.

The text now reads "Over our study period, rainfall rates and deforestation were not related ($p=0.68$ for 5 month dry season precipitation vs. deforestation rates), possibly due to the low climate variability over our study period or because other factors were more important"

Section 4.3, 1st para, 2nd last sentence: fires will also vary to seasonal variation in fire flux, something that could be detected through the atmospheric CO_2 signal. This might be an important way of verifying this model once it has been developed to the pan-tropics.

We agree, and have stated this firmly in the introduction (paragraph 5) as one of the objectives of our work.

Page 3552, last sentence: Please re-iterate here why DECAF biomass estimates are conservative? The reader might not remember here. We have changed the sentence to "Since omission errors on deforestation detection may be more likely than commission errors and because DECAF estimates of biomass are near the lower range of other estimates, our emission estimates should be seen as conservative"

Page 3553, 2nd sentence "lower the uncertainty": Will you decrease uncertainty when it comes to the average, or at what spatial scale. I cannot see why moving to the pan-tropics should decrease uncertainty. I would rather expect it to increase. At least if you look at regional averages of the same size as this one (i.e. at the same spatial scale). Please be more specific and qualify the statement.

We have modified the sentence to be more specific, it now reads "We aim to extend our study area to the pan-tropics to provide a new estimate of deforestation carbon fluxes,

BGD

5, S2696–S2709, 2009

currently a highly uncertain component of the carbon cycle with estimates varying between 0.9 and 2.2 Pg C year-1 [DeFries et al., 2002; Houghton, 2003a; Achard et al., 2004]."

References (new ones added, in press papers now published updated)

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Giglio, L., Loboda, T., Roy, D. P., Quayle, B., Justice, C. O.: An active-fire based burned area mapping algorithm for the MODIS sensor, Remote Sens. Environ., 113, doi:10.1016/j.rse.2008.10.006, 2009.

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Roy DP, Boschetti L, Justice CO, Ju J (2008). The collection 5 MODIS burned area product–Global evaluation by comparison with the MODIS active fire product. Remote Sensing of Environment 112, 3690-3707.

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BGD

5, S2696–S2709, 2009

BGD

5, S2696–S2709, 2009

Interactive Comment