

Interactive comment on “Carbon emissions from deforestation in the Brazilian Amazon region predicted from satellite data and ecosystem modeling” by C. Potter et al.

Anonymous Referee #1

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General comments:

The study tackles the question of disturbance of carbon fluxes in the Amazon by anthropogenic land cover change. This topic is timely and important, as tropical human-induced land cover change constitutes one of the largest uncertainties in the global carbon budget, while at the same time it is discussed as a key component for climate change mitigation. This topic is certainly of interest to many of the readers of Biogeosciences. The method the authors develop for their assessment is new only to some extent, mainly in a new combination of available data and models. Considering the

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huge relevance, however, of quantifying the range of tropical carbon fluxes, this study still constitutes a valuable contribution. The method seems to be clean and logical (the comment posted by D. Morton, however, needs to be addressed), but is not clearly presented. Most of the specific comments below refer to methodological steps that are not clearly or not completely described. The manuscript is further not well structured and needs substantial rewriting to make clear (1) what is new about this study (much of this is mentioned in the ms, but in various places and not prominently enough) and (2) what the relevance of the findings is in the larger context of tropical anthropogenic land cover change and the carbon cycle. I suggest that this article should be accepted for publication after major revisions that take into account the mentioned issues.

Specific comments:

p. 3032, l. 8-11: Make clearer that this is one of the new aspects of your modeling with CASA.

l. 12-16: I have the feeling that absolute numbers of changes in NPP are more relevant for the abstract than the spatial pattern.

Linking "highest" via "whereas" with "more rapid and less seasonal" is not logical - does "more rapid" also imply "higher" here?

l. 19: "Variations in [...] land cover" - is this anthropogenic land cover change or natural changes? If anthropogenic, how does this overlap with "forest burning"? Please define the relationship of anthropogenic land cover change and deforestation (if it is the same, state on p. 3033/3034 that other types like secondary transition from pasture to cropland are not included, and how large the expected error in area is), and which processes of deforestation you investigate: is it (1) forest burning (how are natural and anthropogenic fires distinguished in this case?) and (2) decay of woody debris? Is logging included?

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I. 18-19: Please add WHY old and new CASA studies differ that substantially.

p. 3033, I. 7: The reference list should include at least one of the earlier Houghton (+al) studies, e.g. R.A. Houghton. The annual net flux of carbon to the atmosphere from changes in land use 1850-1990. Tellus, 51(B):298-313, 1999.

p. 3034 State more clearly the new aspects of this study, both concerning method and questions answered.

I. 27: It is not clear where the advantage is of using Landsat deforestation data with MODIS EVI. Would it not be more consistent to use both information from the same remote sensing instrument (both Landsat-derived NDVI and MODIS land cover exist)?

p. 3035, I. 27: You state that NPP declined from 4.34 PgC/a to 4.25 "and then again to" 4.26 - I do not see the second decline.

The reader gets suprised with the reference to Fig.2: it shows the spatial pattern of NPP, not the integrated numbers.

p. 3036, I. 2: NEP "varied from year to year" - this is the expected behavior of a perturbed system. It should be stated here that identifying and quantifying the 'missing' process of disturbance is one of the main objectives of the present study.

I. 5: Define "direct emissions". How can they arise from "preceding years"?

I. 19: It would be clearer to define the light use efficiency as function $e(T,W)$ with a constant factor e_{max} .

Chapter 2 and 3 partially overlap talking about NPP - it might make sense to combine them or shift information (the description of the LBA project may be skipped entirely).

p. 3037: I. 1: A model-data validation for the boreal zone hardly supports the present tropical study. It is more relevant to learn in which respects the validation results supported the model, rather than just learning that the validation has been performed.

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l. 4-5: This sentence is not clear. Is it meant that "T is computed as deviation from optimal temperatures"? 0 degree C appears low as optimal temperature even in the Arctic.

l. 9-13: It is not clear how the land cover class information is included in the model - it is certainly not only used for the moisture stress term, but it does not say so in the ms; e.g. is there no effect of C3 vs. C4 photosynthesis on canopy conductance and assimilation? This would introduce a non-negligible error considering that deforestation for (C4) pasture is a common type of land cover change in the Amazon. Further add here that CASA pursues a fractional land cover approach (currently on p. 3041).

p. 3038, l. 1-3: Soil layer names are inconsistent with Fig.1.

p. 3039, l. 1-5: The neglected disequilibrium due to spatial aggregation is a valuable note. It is probably on a lower order of magnitude than the processes under investigation, but references or first-order estimates should be added that quantify the problem as being small.

l. 14-16: It does not seem logical to readjust the emax-term only for the year 2001, not for the entire 2000-2002 period, especially considering the large difference to the previous value (I assume the new value has been used for the entire 2000-2002 period). It might further be helpful to put this readjustment right with the equation, stating that the 0.39-value had been derived from calibration of AVHRR-derived data, while we are now using MODIS.

p. 3041, l. 25-27: The authors reduce the observed EVI value to represent deforestation. Since EVI is observed, however, and represents a mixture of classes in the 8km gridcell, the EVI for forest types would have to be scaled up accordingly to still match observations on the gridcell basis.

p. 3043, l. 1: Fig. 7 needs to be introduced and discussed before it is referenced here. I do not see new information in the list of hot spots in comparison to Fig.7 except for

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naming the locations. I suggest to remove the list and to leave the spatial description at the following region- and state-level analysis.

p. 3044: The first paragraph of the discussion should rather be part of the introduction to allow the reader a clearer understanding of the processes under investigation right from the beginning.

p. 3045, l. 20-22: A half-sentence description should be added on this previous estimate (to contrast Houghton's method to the MODIS estimate).

p. 3046, l. 5 ff.: This is again confusing the reader concerning which processes are part of the study (pasture vs. crop obviously is) and which are not (logging?).

Fig. 1: If M0 in the first column is indeed meant to be deeper than in the second and third, this needs to be depicted more clearly. Explain all abbreviations (f(WFPS) etc.)!

Fig. 5: A more meaningful label on the y-axis including units will be helpful (e.g. "carbon flux (g C m⁻²)"). It will be clearer if only the location is stated in the title of each plot, not each time again "CASA Deforestation Simulation". Please use larger font for all labels, keys, and titles. The x-axes have 70 months, which does neither match the 10 months in the caption nor the 2000-2002 simulation period of the study.

Fig. 6: Explain "weighted" in the scale unit.

Fig. 7: Please establish the relation between the variable depicted in this figure and NBP.

All figures: The lon/lat labeling is inconsistent - either both lon and lat should be given (after introducing it once) or none. A more homogeneous appearance of the figures would help the reader focus on the relevant content (e.g. a common, either black or white, background should be chosen; the scale unit should either be put in brackets for all figures or for none; same font for all labels).

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Technical corrections:

p. 3033, l. 18: typo: Accordingly; l. 28: typo: estimated

p. 3036, l. 3: one "C" and "of" too much

p. 3038, l. 11: typo: heterotrophic; l. 22 space missing: M1 layer

p. 3039, l. 14: "of" too much

p. 3040, l. 19: "live and dead woody biomass" instead of "live and down dead"

p. 3041, l. 6: split sentence: "[...] after deforestation. Deforestation is mapped [...]"; l. 23: plural: were simulated

p. 3042, l. 2: typo: McWilliam; l. 12 and l. 17: Rh in italics; l. 12: typo: approximately; l. 19: singular: was predicted

p. 3043, l. 19, and p. 3040, l. 22: accent missing on Para

p. 3045, l. 12: word missing: Landsat land cover; l. 17: typo: phenologies; l. 27: add "NBP" to be consistent with l. 3 on p. 3046: total NBP fluxes

Tab. 1: To be consistent with the text and the caption and to be clear, columns 3 and 4 should read "live woody biomass" and "dead woody biomass". The caption should be rephrased to "Predicted totals of standing (live) and down (dead) woody biomass (Pg C) and net biome production (NBP, Pg C yr⁻¹) from CASA model estimates for [...]"

Fig. 1: (a) instead of (I), (b) instead of [II]

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