

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 #2

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General comments:

In their manuscript, Potter et al. present findings from the assessment of the impact of land-use activities-related disturbances such as deforestation on carbon (C) fluxes in the Brazilian Amazon and Cerrado regions from 2000–2002. The manuscript is related to another manuscript recently submitted to BGD (Potter et al., BGD, 6, 947–969, 2009). The central component of their research is the well-documented and established ecosystem model NASA-CASA (Carnegie Ames Stanford Approach), which was applied in a spatially-explicit mode at a monthly time-step. The model was slightly modified to allow for the incorporation of mean stand age, i.e. time since last disturbance,

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which was mapped through interpolation of published biomass data (Baker et al., GCB, 10, 545-563; Mahli et al., GCB, 12, 1107-1138, 2006). The study highlights the importance of decomposition of accumulated woody debris from previous deforestation events (despite the concerns raised by D. Morton, which should be addressed in one way or the other prior to publication) and of direct C emissions from forest burning for the regional C balance. Considering the relevance of disturbances such as (tropical) deforestation and associated land cover changes for the global C cycle, attempts to minimize uncertainty are very important. Thus, the topic of this manuscript is definitely appropriate for publication in Biogeosciences. Length and depth of the manuscript are adequate and the references are generally appropriate. The number of figures/tables is well chosen and both support the text and add to it. However, I fully agree with Anonymous Referee #1 in terms of figure quality and visual appeal, both of which should be improved prior to publication (following the suggestions by Anonymous Referee #1, which I'm not going to repeat as part of my specific comments below). Furthermore, the manuscript would benefit from reorganization towards a more common structure (Introduction, Methods, Results, Discussion, and Conclusions) to make it easier for the reader to distill the relevant information. For example, model modifications are part of section 4 (Prediction of standing forest biomass carbon, page 3039, line 18-25), which mainly focuses on results. The current organization/ structure makes it difficult to identify goals and objectives, results, and especially conclusions that can be drawn. I recommend this manuscript to be accepted for publication after taking into account (at least some of) my general (very important: reorganization of the material) and specific comments.

Specific comments:

[0]: page 3032, line 2: "... of satellite observations of monthly vegetation cover ..." → Rather misleading. The sentence reads as if monthly land cover maps from MODIS were used. [1]: page 3032, line 21: Is "Brazilian Amazon" used for both, Brazilian Amazon and Cerrado when it comes to the results? [2]: page 3033, line 23: Can

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anything be said about the role of changes in CO₂ emissions resulting from changes in land-use practices without changing land cover (similar to Ramankutty et al. (2007); “. . . land cover dynamics following deforestation . . .”)? Maybe as part of the Discussion section? How could these within land cover type changes in biophysical characteristics be accounted for? [3]: page 3039, line 21: Please indicate which Baker et al. (2004) is meant here: two are provided in the reference list. [4]: page 3033, starting line 1: The current manuscript is related to the recently submitted manuscript Potter et al. (BGD, 6, 947–969, 2009). The link between the two manuscripts is clearly outlined at the beginning of section 6 (Discussion, page 3044, line 11-23). Why not establish this link in section 1 (Introduction) and put the current manuscript in context? [5]: page 3036, line 7: Define acronym (LBA) before using it. [6]: page 3036, starting line 12: I think the parts of this section describing the model could/should be shortened since it is almost identical with Potter et al. (BGD, 6, 947–969, 2009). [7]: page 3036, starting line 12: This section should include a description of how the model results were evaluated: NPP and NBP are calculated with the model for sections 4 and 5, respectively, but then there’s comparison with NEP from the LBA program from Potter et al. (BGD, 6, 947–969, 2009). Furthermore, mention that results are assessed qualitatively at the state-level (sections 4 and 5). [8]: page 3039, around line 20: How do other process-oriented ecosystem models account for the age effect? What is the level of sophistication in comparison to other models and ecosystems? [9]: page 3043, line 1-17: What’s the value of the detailed descriptions of the specific locations without a locator map? [10]: page 3046, line 18: What’s next? Very abrupt ending. . .

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