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Discussions

Interactive comment on "Carbon density and anthropogenic land use influences on net land-use change emissions" *by* S. J. Smith and A. Rothwell

Anonymous Referee #2

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This paper presents a simplified model to quantify land use emissions based on prescribed land use maps. The paper consists of a model description, a short comparison of globally aggregated numbers to selected previous estimates, and an analysis of the sensitivity of the results to selected model and data assumptions.

The model and study setup are acceptable, but I am not convinced the paper delivers much new insight scientifically. My concerns are specifically the following:

(1) The model presented just adds one further model to the pool of a few dozens established models that quantify land use emissions. It is a highly parameterized simple model that is based on input data from a large range of different other models and datasets; this may be fine for the sensitivity studies, but inconsistencies between the model input are obvious. The model variables are not evaluated against any obser-

vations, and model results are compared to previous studies only at the aggregrated level of global emissions. The simulated emissions are at the very high end and I would need more analysis, e.g. evaluation against other data at the regional level, to trust this model.

(2) The scientific question answered in this manuscript seems to be "How sensitive is our new model to various assumptions and how well can it reproduce estimates of historical and the RCP4.5 IAM emissions?" This is of interest to those who will use this model in the future, but I am not sure why any other reader, having established, documented, and validated tools at hand, should care about this at the moment. The core of this paper should be an interesting scientific question, not a model description. It should become clear why the authors chose to develop a new model instead of using an established one.

(3) Uncertainties associated with land use emissions are huge and sensitivity studies as performed here are therefore very valuable. However, the motivation behind the selection of which variables to test does not become clear. The sensitivity analysis does not cover the full range of potential uncertainty and the rationale behind testing the sensitivity to specific datasets and not others is not described (e.g., the choice of CASA, CESM, and VEGAS output for carbon densities seems arbitrary). The sensitivity analysis, if better justified, is very helpful and something other publications of new models often lack. But the analysis per se is not a novel scientific question, because the relative and absolute sensitivities depend strongly on model assumptions and are not generally applicable to other land use emission models.

(4) Much of the relevant information on the model is put into the supplemental material. The frequent references to the SM make the paper hard to read. Also, the specific chapter of the SM should be referenced.

To summarize, the paper is the documentation of a model with which potentially interesting studies can be performed (in particular, the authors' good understanding of the

IAM assumptions and how they may differ from biosphere models offers much potential). As such, it is a fine paper, but I doubt that it fits into the scope and aims of BG. I recommend rephrasing the manuscript as a documentation and submitting it to one of the journals that deal specifically with model descriptions (e.g., Geoscientific Model Development).

The following suggestions should be taken into account before publication:

- The study accounts for observed trends in crop productivity and for trends in forest NPP. In particular for the first it is not clear in how far management effects can be separated from the effects of environmental changes. The increase in productivity is likely driven also by factors such as CO2-fertilization. It may be good to discuss this and to add an analysis that excludes all exogenous trends, which would be comparable to a range of previous studies that simulated emissions under constant environmental conditions. It is further not clear how the assumed trends can be applied to the future.

- Sec. 3.3.1, potential vegetation map: If the dataset is corrected with MODIS data, why not use MODIS right away? This points to the issue of ad hoc choices for many of the input data.

- Sec. 3.3.2, cohorts of 50 years length: Please elaborate the effect of cohorts. Usually, cohorts are introduced to models to be able to represent the changes in productivity with age, but for this cohorts need to be split up much finer for the first decades of age.

- Sec. 4.1: P. 4165 explains that emissions are attributed to the ecosystem that loses land, but on p. 4167 it says that land converted to cropland remained a carbon source due to slow equilibration of soil carbon, suggesting the legacy emissions are indeed attributed to the ecosystem that gains land (which would make more sense).

- Tab. 1 should be split into two tables.

- The manuscript reads very well. A few typos/grammar issues are on p. 4170, l. 3; p 4171, l. 20; p. 4172, l. 17-19.

Interactive comment on Biogeosciences Discuss., 10, 4157, 2013.