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Discussions

Interactive comment on "Changes in carbon fluxes and pools induced by cropland expansion in South and Southeast Asia in the 20th century" *by* B. Tao et al.

Anonymous Referee #3

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I reviewed the manuscript "Changes in carbon fluxes and pools induced by cropland expansion in South and Southeast Asia in the 20th century" by Tao et al. and I also read the comments by the anonymous reviewers #1 and #2 after working through the manuscript. As stated by the authors, the manuscript analyses an interesting and relevant field of research. However, I share the concerns of the other two reviewers that claim that the paper lacks an appropriate documentation of essential data sources, methods and assumptions. In my view these shortcomings do not allow a full understanding of what the authors did, how they came to the results and why they arrive at their conclusions. I fully agree with the issues raised by reviewer #1 and especially reviewer #2 and will therefore not list them again. Instead I want to point to some

additional major problems I have with this work.

Method: I have the impression that there is a big imbalance between parts of the method applied. While the authors seem to pay much attention to the details of crop modeling and the parameterization of processes around cropping, the description of other processes e.g. emissions from land conversion are very vague. How can the authors attribute emissions to (forest) vegetation losses without documenting how these carbon pools where initialized? I doubt that details of crop phenology are more important than initial forest carbon stocks when it comes to estimating emissions from cropland expansion over 100 years.

Data: Only a fraction of the data used in this study is appropriately presented and documented. There is almost no information about climate data, I miss parameters of land conversion like emission factors, growth rates etc. I imagine that many of these are endogenously calculated by the model DLEM. But also this model needs parameters to run. For such a large scale application of a very detailed biophysical model I would expect at least two pages of tables with parameters and input data. At least an aggregate of these assumptions should be presented if not the original values. A large part of the data section is essentially a verbal description of the data content that could also go in the first section of results.

Discussion: The manuscript includes a rather long list of literature cited. However, the discussion of results with other studies is rather superficial and not very informative. Naturally differences are due to "differences in study period, data sources and methods". To elaborate on specific differences and trying to attribute them more precisely should be the task of a discussion section. I suggest to shorten the introduction and invest instead into an elaboration of (selected) aspects. This will also help understanding the authors' approach better.

Uncertainties: The authors discuss general uncertainties that are not very exciting as they are more or less trivial and expected from such kind of model analysis. It would be more exciting if the authors would use the model for some simple sensitivity analysis. This would on the one hand enable them to better present their approach and secondly meet their objective of presenting “uncertainties”. There are several parameters that could be varied in their bands of uncertainty and that could deliver interesting results.

Interactive comment on Biogeosciences Discuss., 8, 11979, 2011.