

***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.***

**B. Tao et al.**

bzt0003@auburn.edu

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In our original manuscript, we only addressed the effects of land cover change but excluded the land management practices such as nitrogen fertilization and irrigation, which are tightly associated with the land cover change and associated carbon fluxes. In the revised manuscript, after addressing the referees' comments and suggestions, we extended our study period to 2005 and made a major revision on the manuscript by including fertilization and irrigation effects on cropland carbon storage. As shown in the previous version of the manuscript, the land cover change (without considering land management practices) could result in a source of 0.18 Pg C/yr during 1901-

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2000, which is comparable to the previous estimates by other investigators; however, if we consider land management practices, the land use change could release less carbon, especially in South Asia where land management practices contributed to approximately 30% reduction in carbon emission. Therefore, in the revised manuscript we pointed out that land management practices could play an important role in reducing the carbon emissions due to land cover change in the South and Southeast Asia.

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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.

[Response: thanks for your comment; we added missing description of some data sources and reorganized the Method section (also please see answers to similar questions raised by the 1st referee) in the revised manuscript.]

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)

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vegetation losses without documenting how these carbon pools were 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.

[Response: thanks for your comment; same as most of other process-based models, DLEM initializes carbon pools through equilibrium run driven by environmental conditions in 1900 in this study. Associated processes were well-documented in our previous studies (Tian et al., 2010a, b; Ren et al., 2011a, b; Xu et al., 2010; Lu et al., 2011). We also briefly described associated processes in the revised manuscript].

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.

[Response: thanks for pointing out the issue of input data description; in this study, the half degree daily climate data (including average, maximum, minimum air temperature, precipitation, relative humidity, and shortwave radiation) during 1948–2000 were developed based on data set of NCEP/NCAR reanalysis 1 (<http://www.cdc.noaa.gov/cdc/data.ncep.reanalysis.html>). Since NCEP/NCAR reanalysis 1 data starts from 1948, we used long-term average climate data from 1961 to 1990 to represent the initial climate state in 1900. In this study, we didn't include historical climate change impacts on the carbon cycle. In transient run, the climate condition, together with other environmental factors (i.e. N deposition, atmospheric CO<sub>2</sub> concentration) was kept constant at the level of 1900. This method was also used in our previous papers, such as Tian et al., 2011, Xu et al., 2010 etc.

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In the revised manuscript, we rewrote the model description, in which more detailed information about simulation in carbon changes induced by land use change were given. We also added a table of LUCC-related parameter in the DLEM and reorganized Data and Results sections as suggested.]

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 to invest instead into an elaboration of (selected) aspects. This will also help understanding the authors' approach better.

[Response: thanks for your comment and suggestion; we rewrote the Introduction and Discussion sections in the revised manuscript.]

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.

[Response: thanks for your comment and suggestion; in our revised manuscript, we rewrote the Discussion section and provided some quantitative uncertainties analysis based on our new sensitivity simulations and results from existing studies for this region.]

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