

***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.**

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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 #1 Received and published: 16 December 2011 I don't feel comfortable reviewing this manuscript in public – even anonymously. The paper presents a model calculation of the net flux of carbon from cropland changes in tropical Asia. None of the differences (from previous efforts) that went into this analysis is followed through to the results to demonstrate the effect it had. And in some cases the 'improvements' are not really improvements. For example, the authors haven't justified why higher spatial and temporal processes are more appropriate than coarser ones, or, again, what differences these higher resolutions made. The authors have fine tuned a model, while they haven't touched the main sources of uncertainty. No justification for (or the effects of) the 'improvements' is given. The major justification for this modeling analysis is that it used an updated data set and an improved process-based ecosystem model. But the effects of any of the improvements are not presented.

[Response: thanks for the comments and suggestions. Yes, since our simulated carbon sources and temporal patterns are similar to previous studies (e.g., Houghton and Hackler 1999; DeFries et al., 2002), it seems that our previous manuscript did not greatly improved the understanding of carbon induced by land cover change as pointed out by the referees. So we changed our manuscript structure to emphasize the impacts of the land management practices, which were concurrently changed with land cover change, on the carbon cycle. This revision reflects our improvements compared to previous studies in estimating land cover change effects on the carbon cycle. ]

Major All together they changed the net flux for the entire region a little bit. Further-

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more, there are large mismatches in scale. On a temporal scale, it's not clear why a daily simulation of crop productivity (along with different management practices) is necessary for a 100-year study. On a spatial scale, the case is never made for why the spatial distribution of fluxes matters (page 11, line 12 and following). On a process scale, the model accounts for ecosystem nitrogen and hydrological cycles (p. 7), but there is no mention of what difference those processes have on the estimated fluxes of carbon? And (page 16, lines 15-19), it's not clear how the greater detail for land management and cropping systems changes the end result; that is, the calculated net release of carbon. All of these improvements appear superfluous relative to the huge uncertainty in the rates of land-use change (p. 18, lines 3-15). The authors have chosen to improve aspects that have small effects on the net flux of carbon, and have ignored other data or processes that have large effects.

[Responses: thanks for your comments. We do agree that land-use change has large uncertainties, especially in regional-scale study. That's why we spent much time and effort on developing finer scale, more updated data sets to describe land use change in the study area. In the revised manuscript, we further analyzed the impacts of land management practices. Therefore, our revised manuscript shows the advantages of DLEM in improving our understanding of the consequences of these factors on C fluxes. Since we have no observational or experimental data to prove the improvements of DLEM in the aspects (i.e., better performance with higher spatial and temporal patterns in input data sets) mentioned by the referee, we only emphasize the following improvements in the new version:

1) A number of process-based models have revealed that a model without C-N coupling would largely bias the simulation results. It is the similar case for water limitation (Tian et al., 2011b). DLEM fully couples nitrogen, water and carbon cycles. The limitations of nitrogen and water on NPP, GPP and C storage were simulated;

2) The uses of data sets, land management practices i.e., irrigation, residue returning, and N fertilization etc., improve our understanding of social and economic development

on the cropland carbon cycle.

3) Higher temporal scale (daily time step) simulation processes such as crop growth, C and N mineralization, water evapotranspiration etc. also improve the estimation accuracy for capturing the seasonal or daily responses of ecosystems to multiple environmental factors (including land use change, climate, air pollution, nitrogen deposition, atmospheric CO<sub>2</sub>, etc.), as compared to monthly or yearly time step. Since carbon dynamics in agricultural land are substantially affected by the timing of management practices (such as harvest, irrigation, rotation and fertilizer use), we would not be able to capture the right responses of crop production and carbon accumulation/release during land use process if we lacked these sorts of information in model simulation.

4) DLEM can simulate the temporal carbon dynamics driven by multiple environmental factors (such as climate change, land use/cover change, atmospheric CO<sub>2</sub>, N deposition, air pollution etc.) , while the bookkeeping models used in previous study adopted fixed parameter to calculate the C dynamics related to growth and decay process in each age class.]

The statement that the model builds on other models (p. 6) is too vague a description of the model. For example, how were changes in soil carbon calculated? With processes? As in the bookkeeping model? Related (p. 8, line 12), how do the authors know that a process-based model tracking succession is more accurate than a statistical approach?

[Responses: thanks for your comments; in the revised version, we added more detailed descriptions for the DLEM in simulating C, N and water cycles after land cover changes. We say “a process-based model tracking succession is more accurate than a statistical approach” is in terms of the better performance of process-based approach in tracking daily changes in phenology, water and nitrogen processes, which could further affect the estimations of carbon fluxes. In the revised manuscript, we deleted this comparison since we have no enough observational data as indicators. ]

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Page 12, line 21 and following: the breakdown of the net flux into a land-use, an environmental, and a products component is very interesting. But how do those fluxes compare to previous studies or independent measurements? Different estimates of total net flux may be similar for the wrong reasons. That is, the mechanism responsible for sources and sinks may be very different.

[Responses: thanks for your comments; we didn't compare our results (e.g. the C fluxes induced by land conversion, product decay) due to the lack of independent measurements. However, in our previous studies, we already evaluated DLEM performance by comparing DLEM-estimated different component fluxes (e.g., GPP, Ra, Rh, Ec, etc.) with other studies and independent measurements (Tian et al., 2011c)]

Page 17, lines 10-11: To say that the differences among studies were “attributable to differences in study period, data sources, and methods” isn't saying much. What else is there?

[Responses: we provided more details to explain the sources of divergence in revised version.]

Moderate issues The introduction is too long; it's misplaced discussion.

[Response: we rewrote the Introduction section and made it short.]

Page 9, lines 17 and following: this discussion seems like it belongs in the results rather than in a section on data sources.

[Response: we reorganized this section and moved part of them to Results section.]

Page 11, line 23: what was the source of data for CO<sub>2</sub> and N deposition in 1900 (and throughout the 100 years)?

[Response: in this study, standard IPCC historical CO<sub>2</sub> concentration data (Enting et al., 1994) was used for years before 2003. Annual CO<sub>2</sub> concentration data for years after 2003 were from Earth System Research. Laboratory (ESRL,

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<http://www.esrl.noaa.gov/gmd/ccgg/trends/>); nitrogen deposition data of 1900 was retrieved from a global data set that was extrapolated from a three yearly maps (Dentener et al., 2006).] We added related description in the revised version. Data set. Available on-line [<http://daac.ornl.gov/>] from Oak Ridge National Laboratory Distributed Active Archive Center, Oak Ridge, Tennessee, U.S.A. doi:10.3334/ORNLDAAC/830.

Small errors Page 3, line 3: Tillman, 1999 is not in reference list.

[Response: we added it in reference list.]

Page 3, line 4: Chen et al., 2006: there are two ‘Chen et al., 2006’ in the reference list: They need to be distinguished with an ‘a’ and ‘b’.

[Response: We fixed this issue.]

Page 15, lines 15-17: Suggest: “Our results indicated that 1.56 x 106 km<sup>2</sup> of cropland was abandoned over the 100-year period.”

[Response: We rewrote this sentence as suggested.]

Page 16, line 13: “least area index (LAI)” should be “leaf area index (LAI)”.

[Response: We have fixed this typo in the revised version.]

References: Dentener, F. J. 2006. Global Maps of Atmospheric Nitrogen Deposition, 1860, 1993, and 2050. Enting, I.G., Wigley, T.M.L., Heimann, M., 1994. Future emissions and concentrations of carbon dioxide, key ocean/atmosphere/land analyses. CSIRO Division of Atmospheric Research Tech Paper No. 31. Melbourne. Ren, W., Tian, H.Q., Xu, X.F., Liu, M.L., Lu, C.Q., Chen, G.S., Melillo, J., and Reily, J.: Spatial and temporal patterns of CO<sub>2</sub> and CH<sub>4</sub> fluxes in China’s croplands in response to multifactor environmental changes, *TellusB*, 63B, 222–240, doi:10.1111/j.1600-0889.2010.00522.x, 2011a. Tian, H., Lu, C., Chen, G., Xu, X., Liu, M., Ren, W., Tao, B., Sun, G., Pan, S., and Liu, J.: Climate and land use controls over terrestrial water use efficiency in monsoon Asia, *Ecohydrology*, 4, 322–340, 2011c. Tian, H., Melillo, J.,

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