**Biogeosciences
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.

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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-

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 #2

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The authors present a model study to determine the effect of land cover change in South and South-East Asia on carbon emissions in 20th century. The model used is DLEM. My key concern is the very unclear method used in this paper which makes it impossible for me to judge about the validity of the results and discussion. Firstly uncertainties: The authors state at page 11982 line 25 they want to identify major uncertainties, however, there are no ranges given neither in the tables nor in any graph. I do not think this is acceptable if you explicitly state it as an objective of your paper.

[Response: thanks for your comment; in the revised manuscript, we rewrote the Method section to clearly state the model structure, representation of key processes, and model validity, and reorganized the Discussion section for giving more detailed uncertainties analysis.]

Secondly data sources: The author state they use the Hyde v3.0 database and a potential vegetation map which is a combination of MODIS land cover (no version number and access date given here) and Ramankuttys map (first draft ?) combined with a map of C4 grasses by Still et al and lakes and wetlands by Lehner and Döll. This was all combined and reclassifies to DLEM plant functional types? It is not explained how this was done and why? (p11986, l1 to 15).

[Response: thanks for your comment; in this study, cropland maps during 1900–2000 were developed by aggregating the 5-min resolution HYDE v3.0 global cropland distribution data (Goldewijk, 2001) into gridded data with a 0.5 degree resolution. The data conversion followed a principle that total cropland area in the study region remained the same with the total area calculated from HYDE v3.0 percentage data (Liu and Tian, 2010). For the potential map, we generated it based on multiple data sources due to the following reasons: 1) the classification of global potential map from Ramankutty and Foley (1999) is pretty different from that in DLEM, so we used MODIS Global LAND Cover Type product (MOD12Q1 V004, http://duckwater.bu.edu/lc/mod12q1.html), which is relatively more similar to DLEM plant functional types, to minimize uncertainties associated with the land use/cover reclassification;

2) potential map from Ramankutty and Foley (1999) didn't separate C3 grassland from C4 grassland, which is very important in process-based modeling; so we overlaid Ramankuttys potential map with a map of C4 grassland developed by Still et al. (2003) to determine the distribution of C4grassland. Those classified as grasslands in Ramankutty's potential map, but aren't C4 grasslands in Still's map, are assumed to be C3 grasslands;

3) in the same way, since Ramankuttys potential map didn't include wetland, we identified the wetland area based on the Global Lakes and Wetlands Database developed by Lehner and DÂÍoll (2004), which is the most detailed and the most accurate inventory of the global water resources. We briefly described this issue in the Method section in the revised manuscript.]

I was hoping to get some insight from fig. 2 which has the promising title "Simplified simulations of land-use change processes in Dynamic Land Ecosystem" but does not provide any information about it at all. It is a process chart of the model as far as I understand it. [Response: we rewrote the model description section and give more detailed information about simulations on impacts of land use change in the revised

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version.]

There is no data source for the climate driver given! Why did they use average climate from 1961 to 1990 for the year 1900 and the 2000 data for the daily dynamic? Why did they neglect Ozone for the transient run? Which climate data where used for the transient run which goes until 2000?

[Response: 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.We used the daily pattern in 2000 since globally the year of 2000 was neither dry nor wet. In this study, we didn't include climate change impacts on the carbon cycle. In transient run, the climate condition, together with other environmental factors (i.e. N deposition, atmospheric CO2 concentration) was kept constant at the level of 1900 in order to distinguish land use effect alone. This method was also used in our previous papers, such as Tian et al., 2011, Xu et al., 2010 etc., which are in the reference list.

This study aims to investigate changes in carbon fluxes induced by cropland expansion, so we didn't consider ozone effects in transient run. Also, as we presented in the manuscript (page 11988, Line 13), the data of ozone AOT40 index before 1940 isn't available (Felzer et al., 2005). That is why we didn't include ozone effects in equilibrium run either.]

The authors state product pools in the model with a turnover time of up to 100 years which are directly accounted in the net carbon exchange at site (p11985, l19). It is not clear which data are used to do the partitioning of carbon into theses pools. It is also unclear to me, why they are respired on site? I would expect a significant part of it will leave the area and will be exported to Europe etc.? The author state that biomass is

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burnt on site at the time of transition but don't give a reference about the data source for the amount of this burnt biomass.

[Response: thanks for your comment; we didn't include international trade like crop products exported from one region to another region, which needs to involve an economic model and is pretty hard at this stage. Instead, just as we addressed in the manuscript, in the DLEM, we assume that a part of vegetation carbon may enter product pools with lifetimes of 1, 10, and 100 years and the proportion of these carbon transfer is determined by vegetation type. The scheme has been used by other studies and our previous studies (Houghton et al., 1983; McGuire et al., 2001; Tian et al., 2003; Tian et al., 2011).

Site preparation, such as prescribed fire, is extensively used when land conversion (e.g. forest clearing for cropland) occurs. That is the reason why we assume partial vegetation biomass will be burnt immediately after land use change. For the regional study, the data about burnt biomass is not available. In the revised manuscript, we provided more detailed information about this process in the DLEM.]

The soil data used also provide TOC content which was not used for the simulation! Why? I would assume that the equilibrium carbon content in the soil after spin up should correspond to the value given in the database at least in the order of magnitude and at least for the potential natural vegetation see Smith et al. (2007), Smith et al. (2005).

[Response: thanks for your comment; in the DLEM, soil organic carbon is partitioned into labile, slow, and resistant pools. We didn't use TOC data since it represents contemporary soil carbon pattern, which is inappropriate to drive our study that spans 1901-2005. Yes, the equilibrium carbon content in the soil after spin up is in the same order of magnitude as those from observations and inventory data. In our previous studies, we conducted extensive calibration for initial values of soil and vegetation carbon against observations, inventory data (if available), and estimates from other studies

to make carbon stocks estimations fall into the reasonable range (Tian et al., 2010a,b; Tian et al., 2011a,b; Ren et al., 2011).]

The authors define crop rotations using the MODIS LAI product, but there is no method given, how this was done. I would have expected to see, for example, threshold values and the algorithm used to decide what crop was grown and how many harvests are assumed. This will have a profound effect on the carbon balance and it would be good to be more explicit at this point. There is also no reference given for MODIS LAI and when it was accessed. Given all open issues about the data and methods used I don't see how the results can be justified and more critically how anyone could try to reproduce the method. I think the authors should consider improving this section and addressing the uncertainties in the data sources to allow the reader to understand the validity of their results.

[Response: thanks for your comment; more details about the generation of crop rotations map used in DLEM can be found in our recent published work (Ren et al., 2011). We have applied such a method into historical studies over the globe including Southern United States (Tian et al., 2010a), North American continent (Tian et al., 2010b; Xu et al., 2010), and China (Ren et al., 2011; Tian et al., 2011a, 2011b). We added associated description clearly in the revised manuscript.]

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