

***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 #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. 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 Ramankutty's map (first draft ?) combined with a map

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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). I was hoping to get some insight from fig. 2 which has the promising title “Simpliñqed 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. 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 Ozon for the transient run? Which climate data were used for the transient run which goes until 2000? 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 these pools. It is also unclear to me, why they are respiration 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 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. 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). 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.

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Smith J, Smith P, Wattenbach M et al. (2007) Projected changes in the organic carbon stocks of cropland mineral soils of European Russia and the Ukraine, 1990-2070. *Global Change Biology*, 13, 342-356. Smith J, Smith P, Wattenbach M et al. (2005) Projected changes in mineral soil carbon of European croplands and grasslands, 1990-2080. *Global Change Biology*, 11, 2141-2152.

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