

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 #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. Period. 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.

C4943

Major

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

Furthermore, 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.

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?

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

C4944

for sources and sinks may be very different.

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?

Moderate issues

The introduction is too long; it’s misplaced discussion.

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

Page 11, line 23: what was the source of data for CO₂ and N deposition in 1900 (and throughout the 100 years)?

Small errors

Page 3, line 3: Tillman, 1999 is not 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’.

Page 15, lines 15-17: Suggest: “Our results indicated that 1.56 x 10⁶ km² of cropland was abandoned over the 100-year period.”

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

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