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Biogeosciences Discussions

Interactive comment on "Historical and simulated ecosystem carbon dynamics in Ghana: land use, management, and climate" *by* Z. Tan et al.

Anonymous Referee #1

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General comments

This paper was submitted 4 years after the Liu et al (2004a) publication: "Impacts of land use and climate change on carbon dynamics in south-central Senegal", J. Arid Environments. It uses the same model and asks the same questions, which are relevant scientific questions, this time in Ghana. But the paper raises concerns that require major revisions.

In 2004, Liu et al. considered several management options which have a large impact on soil and vegetation C stocks and fluxes in sub Saharan African countries, such as fuelwood harvesting, charcoal production, and the length of fallow periods. Now the authors of the current paper test the use of different amounts of N fertilizers for the

crops under different climate change scenarios for the 21th century (not evaluated by Liu et al. 2004a); but they do not test any alternative scenarios for other important management options, such as wood harvesting, etc. This is quite disappointing as it would really be necessary in order to quantify which management option of such human dominated ecosystems will play the major role in sequestering carbon under climate change. One would like to know, for example, whether the application of N fertilization at 30 kg N ha-1 yr-1 is sufficient to offset the adverse impacts of climate change on soil organic carbon stocks in case demographic pressure leads to intensified cultivation (shorter fallow periods) and enhanced wood-fuel extraction - which is not an unreasonable assumption. What would the role of management practices like crop rotation or residues removing be? Since this is not addressed, it is difficult to tell that substantial conclusions are reached by the paper.

Since this paper comes several years after Liu et al. 2004a, without presenting any novel concept, idea, or tool it should bring to the reader something more than another application of the same model elsewhere, or it should reflect more about how it compares or not with Liu et al. 2004a. It would also be better to tell why it was decided to test some scenarios of management and not other ones. I would recommend to run all possible management options, especially those already examined by Liu et al. 2004a, such as fallow length, wood-fuel extraction etc., combined with the ones newly examined here (N fertilization), and to also quantify the role of the management options that are now just randomly chosen.

Specific comments

p.2345 line 7: Give one reference for GEMS. It was first described within Liu et al. (2004b). Liu et al. (2004a) used it for an application in Senegal very similar to this paper.

Liu, S., Kairé, M., Wood, E., Dialloc, O., and Tieszen, L. L. (2004a): Impacts of land use and climate change on carbon dynamics in south-central Senegal, J. Arid Environ-

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ments, 59, 583-604,

Liu, S., T. R. Loveland, et al. (2004b). Contemporary carbon dynamics in terrestrial ecosystems in the Southeastern Plains of the United States. Environmental Management 33(SUPPL. 1): S442-S456.

p.2345 lines 19-22: "Freitag et al. (2007) used an isotope mass balance approach to estimate the annual photosynthetic C fluxes over the woodland and savanna-dominated ecosystems of the Volta River basin in West Africa and found that the annual photosynthetic C fluxes are associated with water vapor flux and heterotrophic soil respiration";

The fact that "The annual photosynthetic C fluxes are associated with water vapor flux and heterotrophic soil respiration" is not a result from the study...

p.2347 line 3: as said before, GEMS is better described in the "other" Liu et al. paper (2004b). On the other hand, the lines 3-9 are an exact copy-paste of the text of Liu et al. (2004a), which is quoted by the authors. Therefore it would be more correct to modify the text (lines 3-9) by clearly saying: "As Liu et al. (2004a) said, GEMS (Liu et al., 2004b) was developed for a better integration of well-established ecosystem models with various spatial databases for simulating biogeochemical cycles over large areas. It has been successfully used to simulate C dynamics in vegetation and soil at various spatial scales. The CENTURY model (Parton et al., 1994) was selected as the underlying ecosystem biogeochemical model in GEMS because it has solid modules for simulating C dynamics at the ecosystem level and has been widely applied to various ecosystems worldwide";

p. 2347 lines 15 and following: Several lines are quite close (if not exact) copy paste of parts of the Liu et al. 2004a paper. That could be understandable as this paper presents very similar work, but the citations should be acknowledged clearly. Also the text flow is chaotic, as there are on p.2347 first generalities on GEMS, then some description of the methodology applied for this specific case, then generalities on GEMS

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again. Please give first a brief description of GEMS by clearly referring to Liu et al. 2004 when the text is taken from there. Second when the hypotheses chosen for a model application in Ghana are the same as the hypotheses chosen for a model application in Senegal by Liu et al. 2004b, then this must also be referred to.

p. 2348 line 11: could it be possible to know more on the management data? This was much better documented in Liu et al. 2004b

p. 2351 lines 10-14: The authors refer to 5 studies to support the fact that their computed SOC loss is within the common reported range. However none of the references is really appropriate. The 3 references dealing with SOC change due to land use and land management changes refer to temperate agricultural systems (Donigian et al. 1994, Paul et al. 1997, Buyanovsky & Wagner 1998). The two references that report on carbon flux from tropical deforestation (De Fries et al. 2002, Houghton, 2003) do not provide estimates on SOC content and losses. In their meta analysis on soil carbon stocks and land use change, Guo & Gifford (2002) refer to at least 2 studies on soil carbon content change following a forest to crop conversion in Nigeria: Aweto (1988) and Aweto & Obe (1993). It would certainly be good to look at them, and in any case, to use appropriate references here.

Guo, L. B. and R. M. Gifford (2002). Soil carbon stocks and land use change: a meta analysis. Global Change Biology 8(4): 345-360.

p. 2352 lines 4-9: which cropping system and which management practices were considered within the ensemble simulations of the 20th Century? The different crop rotation and the use of crop residues impact the C cycle a lot. There is generally a lack of information on these points. It does not allow other scientists to compare these results with computations from other ecosystem models.

Fig. 3. I do not understand the Figure. GEMS is run with the hypothesis of only natural vegetation in 1900. So what means the estimation of SOC in 1900 for the cropping system? How shall I interpret the decrease in SOC for the open forest curve?

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I understand that it was "only" open forest in 1900 and that more and more cropland was established within that biome. Does the curve represent a constant area during the whole 1900-2000 period, as I assume? Therefore, the same question again: where comes this cropping system curve from? What does the reduction in 35% SOC for the cropping system represent? Change to crops with lower biomass return to the soil? This should be explained.

p. 2352 lines 18-20: The authors suggest that we expect a generally positive response of photosynthesis to climate change (or elevated CO2). Many studies on crop productivity under climate change give opposite results. Without farmer's adaptation to other cultivars (is this possibility accounted for within GEMS?), warming shortens the crop cycle, therefore decreases the production. Precipitation decreases might limit the productivity even more (although the rather wet conditions in Ghana might reduce such a risk). Several references therefore underline that the positive CO2 fertilization effect will generally only minimize the decrease in crop productivity at low latitudes. The CO2 fertilization effect is also quite much debated, and the reader might want to know how it is represented within GEMS. However it is true that the soil N availability is probably the main factor here.

Fig.4: Ecosystem C is increasing in some places: for which ecosystem does it happen? Why?

Fig. 5d: is the decrease in maize yield under NCC_N4 significant? If so what is the explanation?

Technical Corrections

p.2346 line 5: "the variations of interannual climate" change to "the interannual variations of climate"

Fig.1: not clear what the green & yellow areas are

p 2349 line 3: is (EPA, 2000) also the reference for MAG-

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Interactive Comment

ICC? The document, which should be available on the web site http://www.epa.gov.gh/index.php?option=com_content&task=view&id=54&Itemid=49 is empty at this date (July 2008). Please provide a reference that the reader can access.

p 2349 line 24: the site is http://earthtrends.wri.org/ (and not earthstrends)

Fig. 2: the legibility (e.g. dates) should be improved. It would help to add a map that indicates the natural land cover type in 1900 to better figures out what drives the differences in C stocks between the South and the North of the district.

Table 2 is incorrectly edited

p.2345 line 19: It is "Freitag et al.(2008)";

Fig. 4: The colour of the soil C stock legend must be corrected (red is missing).

p. 2353 line 24: "Adiku et al." 1998 instead of 2004.

Fig. 6: The legend is incorrectly edited.

p.2355 line 2: "(Parton et al., 1994)", instead of "(Parton, 1994)"

Interactive comment on Biogeosciences Discuss., 5, 2343, 2008.