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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 #2

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General remarks

This ms simulates carbon stocks according to climate change scenarios and fertilizer application in a study region in Ghana. This is a potentially interesting subject since the idea of increasing carbon stocks with increased fertilizer application can potentially influence the e.g. the CDM implementation. However the ms has several weak points from the choice of the climate scenario, the fertilisation scenario and the presentation of the results. This paper appears in parts to be a re-run of Liu et al 2004, from which parts of the model description are copied. In the earlier paper a very similar approach was used to simulate carbon stocks in the Senegal. The results of this study are only briefly mentioned and as far as they are mentioned in the current ms, they are also similar. This questions the novelty of this ms. If such a close predecessor

exists, the authors should explicitly (I) state reasons for applying a similar method to a different case study and (II) relate their results in detail to the earlier study to justify this. The main flaw of this ms is that the authors make no attempt to relate their results to a larger scale. It is not clear whether the results are of relevance to a scale larger than the study area. I assume that by relating the mixture of land use / land cover classes or precipitation to the country of continental level, the authors could at least make estimates for which areas their results could be of relevance apart from the study area. Additionally the ms lacks clarity in the description of methods and results and it is sometimes not clear what exactly the figures refer too. Tables as well as figures are of low quality.

Specific remarks

The model description in the cited Liu et al. 2004 is not more comprehensive as in the recent ms and it is not the first publication of the model. The authors should therefore refer to the publication which is cited in Liu et. al. (2004) in Environmental Management of the same year, which has an earlier and more precise model description.

The authors mention that they have 'used the field observation data of the ecosystem C and SOC stocks and the grain yields of major crops as references to verify the corresponding outputs and repeatedly ran model simulations by adjusting parameters after each run until the outputs matched the field measurements as closely as possible' in the 'Uncertainty analysis' section. This fitting should be described in the modelling section. Especially the parameter that needed to be changed should be stated in order to describe the simulation.

The chosen climate scenario is not freely available and if such a scenario is used, its predictions should be related to other more common and available scenarios. Otherwise one can not put this study in context to other studies. As for the chosen fertilization level, a reference of how reasonable these levels are would be needed, e.g. is there a program trying to impose such fertilization levels?

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The study chooses combinations of increased fertilizer application and climate scenarios, which is interesting. However combinations between climate scenarios and fertilizer application are missing, (no climate change and fertilizer increase) which would be needed to see whether the strong fluctuations in the no change scenarios are caused by higher fluctuations in the current climate versus the projected climate or by high sensitivity to climate under low fertilizer input.

The authors write 'the uncertainty of simulations was evaluated along with outputs and expressed in terms of the coefficient of variation'. Does this refer to table 2 and the given stdev? What I am missing in all simulations is a statement whether the results are significantly different from each other or in other words whether the simulated factors have a significant effect. In figure 6 for example it is not clear whether the different climate scenarios have any effect on SOC and if, how does it differ under different fertilization schemes? Since this is a Monte Carlo simulation this should be easy to calculate, and I would also suggest to add error bars to the figures to visualize the uncertainty. Table 3 is intended to show the sensitivity of the SOC and crop yields to the climate scenarios. Here it is not clear to me at all how these values are calculated. Especially: what has been correlated? The yields of different years to the climate or the yields in different cells to the climate? If the first, how can there be a line for the 'mean annual min temperature' and why is there only one value for the soil organic content? Do this values relate to the climate change scenarios run with no fertilizer increase? If yes why is there no line for this combination in figure 6. The message from this table for me is that climate has an influence on SOC, the message from Figure 6 is that climate has no influence. The authors should think of clearer ways to present their results and possible implications.

Missing perspective at a wider scale: The chosen project area may be 'representative for a forest/ savannah transition'. However, it is unclear how the results of this study can be related to a national or continental scale. Has the whole of Ghana a similar distribution of land use /land cover classes? Even if no explicit calculations are done, the

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authors should at least discuss the validity of their main conclusions for larger areas. Table 1 lists percentage changes of LUC between 1972 and 2000 for the study area. If these numbers are compared to national statistics or estimates at the continental level, the authors could increase the value of their results. The study area is treated as a unit, in listing the total precipitation and temperatures. However, looking at the top left figure from figure 2, it looks like there is a gradient of natural vegetation which is then removed by land use. This should be explained further and a map of land use and maybe precipitation is definitely needed to interpret both figure 2 and figure 4.

The summary starts with 'Ghana has distinct vegetation zones from moist forest in the southwest to Sudan savanna in the northeast of the country'. And carries on about desertification and other broad scale issues. However the presented results are only valid for the study area and no attempt is made to relate this to either the whole of Ghana or the continent.

The study reports an increase in Ecosystem C stock nearly similar in all climate change scenarios including no change for woodlands. This result is striking since if there is no land cover change assumed, why should the carbon stock increase with current climate if the system has been brought into equilibrium? This needs further explanation in the text. For woodlands it looks as if projected climate change has no influence at all. Is this reasonable at a broader scale? Would a decrease in precipitation not lead to decreased production? The authors should very explicitly discuss this in context to the choice of the study area and discuss the validity of this particular striking result at a larger scale.

The ms notices a 26 % loss of SOC (measured in Mg C per ha) in the open forest and explains this with deforestation e.g. for cultivation with respect to figure 3. In this case the figure caption as well as the chosen unit is misleading, because it does imply a constant LULC. But in fact, the mentioned line which is labelled as 'open forest' starts as the mean value of cells being 100% open forest and end as a mean of SOC over several LULC. It does not give us the information how the SOC stock changed over

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the time period for a single hectare of open forest that stayed open forest. Here the information on how much SOC was lost due to LULC change (which I assume is the majority) and how much SOC was lost due to other processes like climate is mixed. Such a figure should be labelled Total C loss due to LULC change and should also not be in Mg C per ha but in total C since the area does not stay constant. All figures are in bad quality and figure 4 and 5 are not readable at all. The legends of figure 4 are listed twice. To evaluate figure 4 and 5 two other maps are missing: A LULC map and a precipitation map. Otherwise the cause for spatiotemporal change can not be seen and in this case both figures can be removed without any loss of information since the totals are listed elsewhere.

Technical remarks:

Table 1 lists a code which is not explained. Table 2 and 3 are wrongly set including possibly missing values in table 3. All figures are of low quality, error bars are missing. Figure one lists three areas in Ghana. Why? Caption in the figure is also wrong set. Capitalize legend in figure 3. The caption and the headings of figure 3 are hard to interpret. Are 'Cultivated Savanna' and 'Woodlands' meant to relate to SOC? If yes, state explicitly. There are some mistakes in the text like double spaces or capitalized letters.

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