Biogeosciences Discuss., 11, C7125–C7131, 2014
www.biogeosciences-discuss.net/11/C7125/2014/

Biogeosciences
Discussions
Open Access

Interactive comment on "Modeling the impact of agricultural land use and management on US carbon budgets" *by* B. A. Drewniak et al.

Anonymous Referee #2

Received and published: 4 December 2014

Overall comments:
This discussion paper investigated potential of the effect of crop residue removal on changes in soil organic carbon (SOC) stock in land under agricultural usages, at a large geographical scale. Simulation experiment using CLM-Crop model was applied for this purpose, in combination with spatial and temporal inventories on historical climatic data, land-use/land-cover, and crop-calendar. By using the design of the simulation experiment very similar to that employed in previous study by Drewniak et al. (2013), this study focused on the changes in SOC stock. In addition, validation for the performance of the CLM-Crop model prediction for SOC stock over contiguous USA was conducted using gridded SOC stock data from IGBP-DIS and field observed SOC stock data from ISCN, respectively.

Challenges to improve earth system models to deal with agricultural ecosystems, and especially, SOC stock changes, are of interest for a wide range of scientific community. Especially, the attempt to conduct model validation using field observed SOC data would attract a great deal of interest.

My overall impression regarding to this manuscript is as follows.

1. First of all, some of the authors' interpretations on the result of validation on model performance to predict amount of SOC stock on lands under agricultural use (i.e. model predicted SOC stock vs. observed SOC stocks in agricultural lands in ISCN data; Fig. 3) are rather questionable. Although, the authors postulated that CLM-Crop can capture the SOC stock at various agricultural sites, however, I think it is difficult to conclude that such statement is supported by the results presented in this study. Rather, it should be interpreted that model simulation failed to predict variations in SOC stock at various agricultural fields.

2. In addition, methodology for model validation using the ISCN data was too simple and not appropriate for the purpose and the question addressed. From the experimental setup it is obvious that there is a large gap in the size of spatial entity between model simulation prediction (a $2.8^\circ \times 2.8^\circ$ grid, with varying area of soil columns) and observed soil data set (a field). Therefore, I think authors should build more elaborate strategies to compare observed SOC data with model output by, for example, filling this gap, before concluding just that large variations in field observations made the model validation difficult. The same can be said to the method of comparison for SOC stock between model predictions and observations in ISCN applied in this study, which just simply compared averaged SOC stock of soil columns in a grid, with differing depth (e.g. 0-300 cm or deeper for model simulation; 0-15 or 0-30 cm for most of observations in ISCN; according to the body text), without any attempt to minimize the effect of this difference on the comparison of SOC stock of soil columns by, for example, using uniform depth of soil columns for SOC stock calculation for both model simulation and observations.

3. Although, authors postulated that CLM-Crop model could capture the range of SOC stocks observed in agricultural fields (Fig. 2), however, it is difficult for me to judge whether this was true because of above mentioned reason. I also have questions with regard to 1) reasons why observed data records in ISCN having SOC value greater than 50 kg C m-2 were excluded (Fig. 2 and Fig. 3), and 2) potential bias in the ISCN dataset, if any measures like stratified random sampling had not been employed in sampling sites setup (please see specific comment).

4. I believe that the above mentioned points (1-3) are crucial, as many of subsequent discussions on the size of the effect of residue removal on SOC stock change were based on the advocated good performance of the CLM-Crop model to predict SOC stock in land under agriculture. Therefore, I rate this point as a major flaw.

5. About SOC stock of all land, model predicted SOC stock over all land-use types over USA, 84 Pg C, was found to be comparable with previous estimation by Kern (78-85 Pg C; Kern, 1994). However, more detail explanation on the setup of model input data for historical land-use change is needed to interpret meaning of this result (please see specific comment).

6. In conclusion, I rate the paper is not acceptable in present form, and recommend that the paper should be revised so that it will be evaluated again whether or not it can be accepted for publication in BG. I recommend authors to revise the paper with taking into account the above mentioned points as well as specific comments shown below. I would like to encourage the revision, as a challenge to conduct validation of model performance to predict SOC stock change using field observed data is important and would attract a great deal of interest for scientific community. I included some suggestions for revision.

Specific comments:

Land-use change:

Please add more detail explanation on historical land-use change setup for the simulation. From the body text, it seems that land-use change (i.e. conversion of grasslands to croplands) was set to occur only once throughout the entire time sequence of simulation, and all at once at the end of spin-up (in 1850; if I understood correctly). However, land-use/land-cover data used to assign land-use conversion from grassland to cropland corresponds to early 1990s; e.g. land-use/land-cover dataset used for grass scenario (IGBP DISCover) corresponds to 1992-1993 (Loveland et al., 2000), whereas that for other scenarios represents the early 1990s (Leff et al., 2004). Therefore, I wonder whether the occurrence and duration of cropland land-use was largely overestimated in the simulation. If this is the case, I do recommend revising the input data of historical land-use change to include several land-use change events to be more consistent with the changes in cropland area estimated by Ramankutty and Foley (1999) and to re-execute model simulation with the revised input (I am not sure whether the model can deal with land-use change to occur several times during the course of simulation, though). As the simulated SOC loss in a grid was found to correlate with area of agricultural lands in a grid, this point may be crucial.

Organic matter input to soils:

Input of manure from live-stock waste origin to soils was not taken into consideration in the model simulation. According to MacDonald et al. (2009), about 15.8 million acres of cropland, equivalent to about 5 percent of all U.S. cropland in 2006, were estimated to receive livestock manure. Although, this figure is just an estimate and showing that manure is used on only a small fraction of U.S. cropland, however, for some major crops the percent of the acreage received manure may not be negligible, such as corn (12 %), oats (9 %), as well as hay and grasses (7 %) (MacDonald et al., 2009). Although, input of manure, and in addition, sewage sludge, is taken into account in the estimation of SOC stock change in greenhouse gas inventory reporting of USA, I wonder whether these contribution can be considered as negligible or not. I also think that the title of the manuscript employing the term, ‘US carbon budgets’ is rather exaggerated.

Soil organic carbon stock:

P. 13683, L.6-8:

In "The total stored SOC over all land surface types in the United States, as calculated by CLM-Crop, is 84 Pg C, which falls within the range of previous estimates of 78–85 Pg C (Kern, 1994).", please explain for which year the prediction and the estimate was made, respectively. From Fig. 4, it seems around 2020 is refereed (i.e. 'Current Residue' in 2020 (i.e. 1850 + 170 = 2020) at around 85 Pg C) for the model prediction. If this is the case, I wonder if this corresponds to the year for which the land-use/land-cover estimation by Kern (Kern, 1994) was made.

P.13683, L.28:

What is the reason to exclude the plots with SOC > 50 kg C m-2? Is this because ISCN data has problems in data quality control? Any organic soils included? Please explain.

Fig. 2 and Fig. 3:

I wonder whether the soil sampling site selection in ISCN employed stratified random sampling, with taking different land-use, management, soil types, and climate regimes into account, or not. If not, as is often the case with many of existing soil data set, the data should be dealt in a careful manner especially when the entire data is just simply compared with model output because of potential bias.

Suggestions for revision:

1. Employing calculation for weighted means for SOC stock for each grid based on observed SOC data, of ISCN or including additional soil dataset, with taking into account relative distribution of different land-use type and history, soil types, management practices, etc., in each grid, if possible. Number of data may not be enough to conduct such calculation, though. It would be useful to consult methodologies used for the estimation of SOC stock change at country scale in the national greenhouse gas inventory reporting of USA (USEPA, 2014), which employs an 'expansion factor' for scaling of

SOC stock (stock-change) from observation points to the entire country.

2. Referring to figures of estimated SOC stock (stock-change) shown in the national greenhouse gas inventory reporting (US-EPA, 2014) and to compare it with that predicted in the model simulation in this study. Such information, and, in addition, corresponding discussions will be useful for readers.

3. Use of information on land-use/land-cover, land-use history, and management practices of each sampling site in the ISCN data set when assessing the range, mean, or median of SOC stocks, if such data are available. Again, uniform depth of soil columns should be used in the calculation to compare it with model simulation output. As this study emphasized that differences in crop residue input to soils would have affected changes in SOC stock significantly, such information on land-use (and management, if available) should be included in the method for model validation.

4. In addition, assessing differences in SOC stock between different land-use types (e.g. cropland vs. grassland) at several different geographical zones having different climatic conditions (temperature, precipitation etc.), in model and observation, respectively, followed by making comparison between these two to explore model performance. I expect such approach would produce output that may highlight strength of the study applying earth system model and spatial and temporal inventories of climate and land-use over large geographical entity.

References:

Bonan, G.B., Levis, S., Kergoat, L., Oleson, K.W., 2002. Landscapes as patches of plant functional types: An integrating concept for climate and ecosystem models. Global Biogeochem. Cycles 16, 5–1. doi:10.1029/2000GB001360.

Drewniak, B., Song, J., Prell, J., Kotamarthi, V.R., Jacob, R., 2013. Modeling agriculture in the Community Land Model. Geoscientific Model Development 6, 495–515. doi:10.5194/gmd-6-495-2013.

Leff, B., Ramankutty, N., Foley, J.A., 2004. Geographic distribution of major crops across the world. Global Biogeochem. Cycles 18, GB1009. doi:10.1029/2003GB002108.

Loveland, T.R., Reed, B.C., Brown, J.F., Ohlen, D.O., Zhu, Z., Yang, L., Merchant, J.W., 2000. Development of a global land cover characteristics database and IGBP DISCover from 1 km AVHRR data. International Journal of Remote Sensing 21, 1303–1330.

MacDonald, J., Ribaudo, M., Livingston, M., Beckman, J. and Huang, W.: Manure Use for Fertilizer and for Energy: Report to Congress., Administrative Publication, AP-037, [online] Available from: http://www.ers.usda.gov/publications/ap-administrative-publication/ap-037.aspx, 2009.

Ramankutty, N., Foley, J.A., 1999. Estimating historical changes in global land cover: Croplands from 1700 to 1992. Global Biogeochem. Cycles 13, 997–1027. doi:10.1029/1999GB900046.

U.S. Environmental Protection Agency, 2014. Inventory of U.S. Greenhouse Gas Emissions and Sinks: 1990-2012 (No. EPA 430-R-14-003). U.S. Environmental Protection Agency, Washington.

Interactive comment on Biogeosciences Discuss., 11, 13675, 2014.