

Interactive comment on “Evaluating the interaction between sediment fluxes, carbon dynamics and biomass production using an integrated model” by S. Bouchoms et al.

Anonymous Referee #2

Received and published: 10 September 2018

GENERAL COMMENTS

The interdependencies of erosion and soil carbon balance have been investigated in many model-based studies. In a next step, vegetation should be explicitly included in integrated simulation approaches. Therefore, the authors tackle a relevant topic.

The general approach of the study is suitable to investigate the interdependency of erosion, plant growth and soil carbon balance. Nevertheless, the implications of the chosen implementation are not clearly addressed and are not sufficiently considered in the interpretation of the results. The study contains several flaws, which need to be addressed by the authors to make the results publishable in SOIL (see below).

C1

In addition, the manuscript is not carefully prepared, hard to read, and hard to understand. This is mainly due to the lack of a common thread and to the fact that the authors use different terms for the same thing throughout the manuscript (e.g. net flux and cumulative flux and vertical flux, erosion and soil truncation, etc.). The mathematical notation is not clear and does not follow a general concept. Therefore, the text requires a complete revision to make it suitable for a scientific journal.

SPECIFIC COMMENTS

In the following paragraphs, I address the main problems I found concerning methodology and presentation of the study:

The study applies a very simple SOC model together with an equation that relates yield to erosion and an approach to translate this into depth-dependent carbon input to the soil. From my point of view, this model must not be labeled "integrated", since that would require a plant growth model. Therefore, the title of the paper should be changed. The same applies to the statement the model would dynamically link crop yields, soil properties and SOC dynamics. The model does not contain a dynamic link from soil properties to crop yields but a static assumption on the effect of erosion.

The authors use two scenarios, one of which they call FB (feedback). However, Fig. 2 and the model description reveal, that actually there is no feedback loop in the model. Using the term "feedback" is therefore misleading. I suggest using a term like "yield effect".

A central point of the study is that agricultural yield changes with soil truncation. However, there is no direct link between these variables. Soil carbon input depends more on total biomass than on yields. However, the fraction of the harvested plant organs from the total biomass (harvest index) is physiologically controlled and therefore it is variable. As the authors point out, there can be different causes for the effect of erosion on plant growth. In the real world, farmers take measures to compensate for these

C2

effects. These simplifications need to be addressed when describing the general approach of the study and have to be included in the discussion. In this context, objective iii where the authors state their intention to investigate long-term effects of erosion on crop growth also needs to be rewritten.

In the results, the authors present data on relative yield. Here, an explanation on how the reference value was set by Bakker et al. (2004) is missing. This is crucial in order to assess the results.

The model description is hard to understand because different terms are used for the same thing (e.g. input from crops vs. flux from the atmosphere). It requires a more precise presentation. In addition, the following points have to be addressed:

- the timestep of the model has to be given
 - equation 2 and 3: If $h \neq 1$, where does $(h - 1)k_y r Y$ go? This is only implicitly stated in Eq. 9
 - using 100 soil layers seems very detailed compared to the very general assumptions on vegetation effects and C input from roots. Why did the authors choose 1 cm for layer thickness?
 - Eq. 9: it should be stated, that this is just the sum of equations 2 and 3. One could factor out r , which would also simplify Eq. 3
 - values for δ , k_{yt0} , and k_{ot0} are missing
 - Eq. 4 contains manure input, however there is no further information on this
- It also remains unclear, how soil truncation is modelled. Are layers removed from the top? Are the properties of the existing layers altered while keeping the overall soil depth constant? This has to be presented (considering the proposed effect of soil depth on plant growth).

The next point concerns the model validation. As far as I understood the text, the same observational data was used for validation and calibration. If I got this wrong, clarifying text has to be added. If I am right, this is not a validation but an evaluation. Nevertheless, a validation is required and can be accomplished by using a leave-one-out or

C3

bootstrapping approach. As I also commented in the context of the long-term experiment, the validation needs to be conducted with the same set of perturbed parameters as the following experiments. The text states that Fig 3 shows a comparison of simulated SOC content and observations. This comparison should also include uncertainty information resulting from the 1000 simulation runs with perturbed parameters.

After the model validation, the reader will be interested in results of the model runs. How does SOC and C-exchange with the atmosphere develop over time? The authors should present timeseries that enable the reader to get an idea of how the model works. If the data were available, a comparison to observations would be desirable.

Concerning the long-term experiment, it remains unclear, why a second set of perturbed parameters was generated. In order to evaluate the results, the experiments have to be conducted with the validated model and the same sets of parameter values. In addition, information on the scientific basis of the choice of value ranges for the parameters is missing. This is of great importance if the intention of the FAST analysis is to compare the tested parameters regarding their influence on the overall variability. This is because the value ranges used for the parameters have an effect on the resulting explained total variance. In order to interpret the FAST results in the way the authors do, it has to be argued why the value ranges are comparable. Using the same relative ranges is not appropriate due to different relative ranges of the respective parameters in the field. An appropriate method is to use published ranges of observed values together with estimates of uncertainty. If these are not available, reasons for the estimates of plausible ranges have to be given.

It also is not clear to me, which set of model runs was used for the analysis in sections 3.3 and 3.4. Is this based on the same results as the FAST analysis?

Finally, the study requires a comprehensive discussion on the transferability of the results to the real world. Especially the implications of the simplifications in the model on the transferability have to be dealt with. In addition, the authors should discuss the

C4

role of the farmers adjusting their choice of crops, management practices and harvest residuals, etc. This is tackled shortly in the final sentences of the discussion, but this is not sufficient. Other important points to be discussed are the dependency of yield on plant growth, on nutrient availability, and on access to water. All this can alter the harvest index and therefore the relation of soil carbon input and yield.

In the beginning of the discussion, results are compared to Berhe et al. (2005), which, in contrast to the present study, found a carbon sink. Explanation is required why this is rated as a support of the new results.

In the final paragraph, the authors reveal, that with $B > 1.1$ there was no effect on yield. If this is the case throughout the study, the manuscript can be simplified by stating this in the beginning and removing this aspect in the results section.

DETAILED COMMENTS AND TECHNICAL CORRECTIONS

Use the same font and italics for symbols in equations and text unless there is an explicit rule given by the journal.

Use scientific notation for units

Improve the graphical quality of the figures.

In the following I use p for page and l for line:

p114: point (i) accounting for something does not change SOC fluxes but the estimates thereof

p1122: why negative numbers for an increase in SOC losses?

p214f: soil-atmosphere exchanges are part of the carbon cycle. They are not its drivers, which are, by definition, external. In addition, do not exclude vegetation. Its importance is explained in the second part of the sentence.

P219: incorrect format of references.

P2123: typo: photosynthate

C5

p2133: no enumeration beginning here, remove (i)

p3116: numerus: meta-analyses; experiments

p3118f: Bakker is not cited correctly; compare to p3127f

p3114: this is a meta-analysis, not an analysis of meta-data

p413-4: unclear why a clay-fraction can replace explicit accounting for soil depth

p511: there are two van Oost 2005 papers in the references. Please specify. The same applies to some references to van Oost et al. (2007)

P5126, Eq. 7: K has to be lower case since rates were introduced lower case in Eqs. 2 and 3. In Eqs. 4, 6, and 9 dependency on time and/or layer is denoted by t and z in parentheses. Therefore: $k(t, z)$. In addition: explain to the reader that this is used for k_y and k_o

p5127: Sentence incorrect

p5130: refer to equation 4

p5131: to make it easier for the reader to understand the overall model setup, state the source of the cumulative soil truncation data

p719: two instead of 2

p7113: remove second full stop

Table 1 a: What does "period of cultivation" mean? A single number does not define a period.

Table 1 a: table gives an erosion while caption states SOC loss. Avoid this contradiction.

Table 1 a: the caption mentions data for two simulated scenarios, which cannot be identified in the table. In addition, site description and results should not be in the same table

Table 1: scenarios are mentioned in the caption before they are mentioned in the text. In addition, when they are introduced in the text, they are not called scenarios.

p812: parameter sets (not parameters); the same at several positions later in the text

p817: scenarios (instead of abbreviations).

Figure 3: Again the terms: in the figure it is calibrated and observed, in the caption it

C6

is measured and simulated

p10l2: where do the years come from? Were these the same for each site? Is this somehow connected to the periods in table 1?

p10l3: what is the "period of interest"?

P10l5: the parameter sets were not obtained by calibration. This only applies to the mean values.

P10l10: You investigate the feedback effect in the model. This is not a potential effect. Only transferring it to the real world makes it potential.

P10l10: what does the "c." mean?

Table 2: use the same symbols as in the text; φ was introduced as a carbon input profile, not a root density profile. This also applies to p13l13 and p13l16

p10l16-18: sentence unclear

p11l11: instead of "typical values", state how the numbers were computed

Figure 4: consequently use upper or lower case letters to address the graphs of the figure

p12l1: the highest observed SOC loss is said to be 0.19. However, in Fig 4 a, the highest red circle is slightly above 0.2.

Figure 5: If the same variable is on both y-axes, the axis labels have to be the same.

Figures 5 and 6: When comparing simulation and observation or results from different scenarios, the graph should be square.

P19l5: Bouchoms et al. (2017) missing in list of references -> use a reference managing software to avoid this

P19l6: this is a nice explanation of the possible interaction of processes. The authors should consider presenting this in the introduction.

P19l20f: sentence unclear

P22l30: Typo: Impact of

Interactive comment on SOIL Discuss., <https://doi.org/10.5194/soil-2018-23>, 2018.