Reply to the reviewers

Below, we have copied the relevant sections of the two reviews (copied sections are given in black, italic font). We have addressed the reviewers' comments separately, and have given our reply below each copied 'reviewer' section (reply is given in blue font).

Anonymous Referee #2

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).

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.

The necessary clarifications on terminology and mathematical notations will be made in the final manuscript.

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 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.

We welcome this assessment.

In the current version of the model, there is no explicit link between soil properties and crop yields. Our study is based on a published relationship between soil erosion (expressed as soil truncation) and relative yield and implicitly contains these effects. The soil properties, SOC dynamics and C input (derived from yields/biomass productivity) are integrated as SOC dynamics depend on clay content and C input which are influenced by soil erosion. These links are therefore explicit. We will thus reformulate carefully the title and the text so that the difference between explicit and implicit links are clear. The feedback term will be adapted to clarify that it represents the indirect effect of erosion on SOC dynamics through yield reduction.

We agree that yield and biomass production are two different concepts, which are often mixed in the literature and common language. In this paper, we are talking about biomass productivity in response to soil erosion and we agree that farming practices will try to cope with declining biomass production. We will clarify in the implementation and discussion that we are assuming constant agricultural management practices.

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.

We selected data from comparative plots in which the original studies compared yield obtained in non-to slightly eroded soil with yield in eroded soil. Relative yields were calculated as following: relative yield is set to 1 for the non-or slightly eroded soil and fractions of that for yields on eroded soils. Hence, a relative yield of 1 indicates that there is no change in the yield, values < 1 represent yield losses and values > 1 yield gains. We will clarify this in the text and figure captions.

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 $(h - 1)k_{yr}Y$ 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

The model time step is 1 year, we will add this information.

Equation 2 and 3: the quantity (1-h)*k*r*Y represents the mineralized/respired fraction leaving the young C pool.

We used 100 soil layers to have a very fine representation of the vertical soil profile and advection in response to soil erosion. We found that the model was sensitive to the vertical SOC profile and using a coarse resolution resulted in substantial numerical dispersion and smoothing. In addition, as the model computational performance was very good, there is no need for a low vertical resolution.

Eq. 9 is the sum of both equations. However, Eq 2 and Eq 3 are the classic way to present ICBM equations. We refer to Andren and Katterer, 1997 and SPEROS model presentations by Van Oost et al. (2005), Dlugoss et al. (2012), Nadeu et al. (2015).

Values for ky and ko are respectively 0.8 and 0.006 yr-1 and δ is 2.91 (dimensionless). These values will be added to the manuscript.

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).

Soil truncation is modelled by removing soil properties from the top of the profile. Assuming a constant bulk density, the considered depth does not change over time but the soil characteristics are advected upward in response to soil erosion. Soil properties are upward in proportion to the amount of soil removed (see e.g. Van Oost et al, 2005, Dlugoss et al., 2012, Nadeu et al. 2015). At the bottom of the profile, a constant boundary condition is assumed and its properties are progressively included in the soil profile proportionally to the amount of removed topsoil, resulting in an effective truncation of the soil profile characteristics.

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 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.

We agree that the term model evaluation is more appropriate. During this evaluation, we performed site-specific simulations as SOC parametrization, clay content, erosion rate and length of the simulations were specific for each site from the Van Oost et al. (2007) paper. For each of the 10 sites, we created a set of 1000 scenarios for which parameter values were randomly chosen in a narrow range around their published values in Van Oost et al. (2007). These values, associated ranges and lengths of simulations are given in Table 1a. We will add the uncertainty in SOC profiles calibration to Figure 3.

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.

The time-series resulting for our simulations are available and could easily be included in the paper. SOC stock evolution is following a classic two phase evolution: the profiles lose quickly a large amount of carbon during the first decades, and the rate of SOC loss is then decreasing over time due to the lower SOC content of the exposed subsoil. When the yield effect is weak, a steady-state is observed whereby the laterally exported SOC is replaced by new C coming from plant inputs. When the yield effect is strong, it takes a longer time to come to steady-state SOC stocks or there is no steady-state.

To our knowledge, long-term observational data on C fluxes nor SOC stock evolution are not available in literature.

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.

The model evaluation was done comparing the model predictions against observations using site-specific data. These data are displayed in Table 1 and Figure 3. We added a relative uncertainty range around these observations to account for natural variability and errors in measurements at the site-scale. A range of B exponent was attributed to each site, in line with each site's description of soil depth description and climate type. For each individual site, we generated 1000 sets of parameters, which values were inside the range of this specific site. We performed 1000 simulations which time length was site-specific (i.e. 1000 simulations with the parameters of Belgium 1 site, 1000 simulations with the parameters of UK site, etc.). Therefore, the resulting SOC losses and vertical C fluxes can directly be compared to the observed values as the erosion and SOC parameters were close to the observations.

The long-term experiments were performed on the total range of the observed parameter values regardless of the sites considered in the model evaluation: i.e. from the smallest value to the highest value found in the table, with the notable exception of erosion rate, which range was extended further based on erosion data across Europe and the USA. We generated 1000 sets of parameters based on this total range of values (as presented in table 2). Specifically, the range of the yield-effect exponent was chosen to cover the whole set of yield values per unit of soil truncation as extracted from Bakker et al. (2004) and this was presented in the first part of the manuscript. The root-depth parameter indicates the root penetration in the soil and its value was taken so that 95% of the roots are distributed in the first 35 cm to 65 cm with respective values of $\varphi$of 4 to 6, with 30 to 45% in the first 20 cm. These values are in accordance with previous SPEROS parametrization obtained by inverse modelling (Dlugoss et al., 2012, Nadeu et al, 2015). As for the mineralization distribution, the given range indicates a turnover rate at 1 m depth of 137 to 700 years for the slow C pool which is in line with the centennial turnover rate found in deep colluvium by Wang et al. (2014) or Van Oost et al (2012).

We thus argue that the interpretation of the SOBOL/FAST analysis is valid and we will more clearly identify in the text where the ranges of the parameters come from.

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?

The results for the long-term simulations (200 years) in section 3.3, 3.4 are based on a set of 1000 scenarios randomly chosen in the range of values specified in Table 2a. This set was also used in 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 role of the farmers adjusting their choice of crops, management practices and harvest residuals, etc. This is tackled shorty 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.

We will add and clarify the aspect related to the model limitations and the agricultural practices adaptation in the extended discussion about the study limitations. As for the dependency between yields, nutrient availability or water availability, these aspects have been discussed by Bakker et al in their review (2004), Christinsen and McElya (1988), Lal et al. (1999) or Larson et al. (1985). For example, Diaz-Zorita et al. (1999) pointed out that in absence of water limitation, nutrient limitation could reduce yield by 40 $kg.ha^{-1}$. We consider that a more detailed analysis of the biological effects is outside the scope of this paper.

In the beginning of the discussion, results are compared to Berhe at 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.

Berhe et al (2005) found a carbon sink related to the C uptake from the atmosphere occurring in eroding areas. Our study found that erosion can result in a carbon sink (in terms of vertical C fluxes) as the balance is often positive with C being added to the soil. Our study however emphasizes that this C uptake can be overestimated in modelling studies if the long-term evolution of the yields is omitted.

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.

We refer to our reply to reviewer#1. The main goal of our study was to explore the effect of biomass productivity decrease on SOC losses and vertical C fluxes. We acknowledge that the model is relatively simple and required assumptions about the relationship between the C input and the biomass productivity. We agree that the relationship between C input and biomass can be dependent on the amount of residues left on the field but under the absence of data, it is difficult to correctly represent this.

DETAILED COMMENTS AND TECHNICAL CORRECTIONS

We thank the reviewer for the suggestions. For the sake of clarity, we only answer individual comments relative to understanding, clarifications, and precisions. All other comments about typo, references or re-phrasing which do not required detailed answers will be addressed in the revised manuscript.

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

The journal asks for equation symbols to be in italic when used in the text. The necessary changes will be made.

Improve the graphical quality of the figures.

We will improve the figures.

p1l22: why negative numbers for an increase in SOC losses?

This is a mistake as the numbers represent the relative C stock changes. We will correct this.

p3l14: this is a meta-analysis, not an analysis of meta-data

We agree with this comment will change it throughout the manuscript.

p4l3-4: unclear why a clay-fraction can replace explicit accounting for soil depth

The clay fraction can be given in absolute terms, i.e. the volume of clay in a given volume of substrate (soil + rock fragment) or in relative terms as the fraction of clay in the remaining space, not occupied by rock fragments. In our case, the clay fraction is accounted for in the model by the absolute volume of clay per volume of substrate. We further assumed that the relative fraction of clay in the remaining space and the bulk density of the soil are constant. Hence, the absolute amount of clay indirectly indicates how much rock fragment is contained in the substrate, which is a proxy of soil depth as 100% of rock fragment is representing the bedrock level. In the case of deep soft rock, the absolute clay content shows little variation between the topsoil and the bottom of the profile. In the case of physical hindrance, the clay content is highly reduced at the bottom of the profile.

p5l1: there are two van Oost 2005 papers in the references. Please specify. The same applies to some references to van Oost et al. (2007)

We will make the changes

P5l26, 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 ky and ko p5l27: Sentence incorrect
p5l30: refer to equation 4

We will make the changes

p5l31: to make it easier for the reader to understand the overall model setup, state the source of the cumulative soil truncation data

We will add information about the link between erosion and cumulative soil truncation (which is the annual erosion rate * time, as erosion rate does not vary).

p7l9: two instead of 2
p7l13: remove second full stop

We will make the changes

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

The period of cultivation is the total duration of cultivation between the start of cultivation on the considered field and the date of the final analysis. We will correct the header to “time since start of cultivation”

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

The caption was wrong and will be corrected according to the content of the table.

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

In this case, these are different from the period of cultivation as the ^{137}Cs was released in the atmosphere and deposited after the nuclear bomb testing. In the literature and following Van Oost et al. (2007), we took 1954 as the standard date of ^{137}Cs deposition on the earth surface (Ritchie and McHenry, 1990). This date is considered to be identical for all sites. As the erosion rate derived from ^{137}Cs tracer were valid for the period post-1954, the integration of cumulative vertical C fluxes was done over the period from 1954 to the date of the C inventories realized in each individual site rather than over the entire period of cultivation.

p10l3: what is the "period of interest“?

This is the period of cultivation for SOC losses or the period between 1954-date of sampling for the vertical fluxes. We will clarify this in the text.

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

Correct, we will change this in the text

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.
Correct, we will adapt this

P10l10: what does the "c.“ mean?

c. stands for “calibrated” years.

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

We will check the use of the symbols.

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

These numbers of SOC losses were obtained by calculations based on the data provided by Van Oost et al. (2007): stable profile SOC stock, lateral SOC fluxes, vertical SOC fluxes and erosion rate for each site. The total C losses was calculated by integrating lateral SOC fluxes, vertical SOC fluxes and calculating a mass balance to obtain the total SOC lost over the cultivation period. The observed relative SOC loss is the ratio between the total SOC loss and the observed SOC stock. We clarify the method in the manuscript.

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

We will adapt this.

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.

We apologize, this is a mistake in the text.

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.

We will make the necessary changes.

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

We will carefully check the formatting of references, and make sure that the list is complete.

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

Thanks for this suggestion.

P19l20f: sentence unclear

We will clarify the sentence which is describing the three processes involved into the C sink resulting from erosion.