

Interactive comment on “SOM dynamics and erosion in an agricultural test field of the Clear Creek, IA watershed” by C. G. Wilson et al.

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Authors response to interactive comments on “SOM dynamics and erosion in an agricultural test field of the Clear Creek, IA watershed” by C. G. Wilson et al.

The authors have responded to the interactive comments provided by the anonymous reviewers. The reviewers' comments are black with the responses by the authors in red.

Anonymous Referee #1

1. Overall conclusion: this manuscript has a very poor structure. In its current form it is not acceptable. After reflection on the real focus and objective a complete overhaul

could be done but this is even more than major revision and rather a completely new manuscript. Novelty claimed by the authors in the abstract is: 1) impact of spatial variabilities 2) combining the Water Erosion Prediction Project (WEPP) and the CENTURY SOM dynamics model

Reviewer 1 notes that the paper has poor structure and suggests that there is no real focus or common objective for the manuscript. The main theme of the paper was mentioned three times (at the end of the introduction, at the beginning of the results, and the beginning of the conclusion). Each time the phrasing was different, which may have led to some confusion. In the Introduction, the main objective involved understanding “the spatial distributions of SOM as controlled by soil loss and deposition resulting from historical and current management strategies”. In the Results, the foci of the paper were identified as “the changes in SOM dynamics resulting from shifts in different management practices and the effects of utilizing deposition rates in SOM evaluations.” The spatial component was apparently dropped. Finally, at the beginning of the conclusion, the text mentions “the role of spatial and temporal variations of erosion/ deposition on SOM.” The spatial component was present, along with a new temporal component.

The authors have refocused the paper to discuss only the importance of accounting for deposition in SOM redistribution studies. Dealing with only erosion will inflate loss rates from a field. The authors have removed references to spatial and temporal distributions. The above passages in the Introduction, Results, and Conclusions have been altered and are now as follows:

Introduction: “the main objective of this study was to better understand the need for more accurate accounting of the spatial redistribution (i.e., soil loss and deposition) of SOM as controlled by historical and current management strategies.”

Results: “The foci of this study were the changes in SOM concentrations in a test field resulting from shifts in different management practices and the effects of utilizing deposition rates in SOM evaluations.”

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Conclusion: "To date, few studies have examined in detail the role of erosion (more specifically soil loss and deposition) on SOM, i.e., deposition is still poorly understood."

2. Section 1 "Introduction" lacks structure and needs a very fundamental overhaul. A more systematic review of different aspects separately followed by a review of their interaction would be more readable.

Reviewer 1 notes that the introduction lacks structure and suggests a more systematic review of different aspects. The authors refocused the introduction to systematically follow the components of Equation 1, which is a simple budget for the SOM concentration at a specific place and time. In the Eq.1, the three constituents are external inputs, decomposition, and erosion. These three constituents are affected to different degrees by soil properties, climate, and applied management practices. The introduction explores each constituent separately and examines how it is affected by each control. This discussion ends with erosion, which encompasses four stages of detachment, transport, redistribution, and deposition. The effects of erosion (namely deposition) on SOM are understudied relative to the other two constituents of Eq. 1. The remainder of the Introduction elaborates on the need for more knowledge regarding the effects of deposition on SOM.

3. A lot of statements are true but hold little or no information. The authors attempted to identify these ineffective sentences and either removed them or adjusted them to be more substantial. 3a. Example of such sentence (pg 1583 line 14-19): "The constituents of Eq. 1 are controlled by interrelated driving forces within the critical zone (Fig. 1). The relative influences of the individual controls differ depending on different land uses, landscape positions, and scales, at which they are studied. Moreover, many aspects of these interactions are grossly understudied, which inhibits overall understanding of the processes occurring in the critical zone (Chorover et al., 2007)." What does this sentence contribute?

The above passage was altered to remove the ambiguous commentary and discuss

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directly the driving forces behind equation 1. It now reads as follows:

"The constituents of Eq. 1 are controlled at the hillslope scale by both intrinsic soil properties and extrinsic driving forces, namely climate and applied management practices, which are further discussed herein."

3b. Another sentence (page 1584 line 14-16): "Now, a strong relationship exists between erosion and SOM loss (Starr et al., 2000; Papanicolaou et al., 2009), so it follows that SOM concentrations are also strongly influenced by the applied management practices." In this sentence an oral style is used and secondly the sentence adds very little or no information.

The above passage was altered to remove the conversational tone. The sentence is now used as a transitional sentence from the introductory discussion on the major constituents of Eq.1 to the focus of the paper, which is the need to account for deposition in SOM studies. The new sentence now reads as follows:

"Regardless of the role of certain management practices on erosion, there exists a strong relationship between erosion and SOM redistribution/ loss."

4. Equation 1 has only time as independent variable and therefore should not be written as the equation for spatial distribution of SOM. This equation represents the balance at one point, which is a rather trivial equation. Moreover equation is its form is not a partial differential equation and should therefore not be written with the Greek delta but with the ordinary "d" as in an ordinary differential equation. See later also equation 2 which suffers in the same way.

Equation 1 may be simple; however, the authors do not feel it is trivial. It presents the key controls on SOM and provides the background for the study. Erosion is a key component; however, it is not properly accounted in most SOM studies. The Greek letter "delta" was replaced with a lowercase "d" as suggested. The sentence was altered to clarify that this budget is for a specific point: "The concentration of SOM (S) at a

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specific place is quantified most simply through the following budget:"

Equation 2 in the original manuscript was removed while shortening the methods in response to another comment.

5. In the section 2 "Materials and methods" the Century model is described in §2.1 followed by Model simulations (which includes reference to the test field of the "Clear Creek, IA" in §2.2. After that comes the "CENTURY inputs" in §2.3 and then in §2.3.1 the description of the study site. In other words the structure/sequence is wrong. Better could be to describe the model, then the "input" as required in general, followed by the description of the study site and then the results of the simulation.

The reviewer suggests restructuring the methods to proceed as model description, general model inputs, study sites, and results. The authors rearranged the methods to the following:

2.1 Model Description 2.2 Model Inputs 2.2.1 Study site and site specific parameters
2.2.2 Climate 2.2.3 Test field management strategies 2.2.4 Erosion 2.2.5 SOM loss 2.3 Model calibration and verification 2.4 Model simulations

The Methods section begins with the model description and general inputs. The section then focuses on the specific input related to the study site (including soil information), climate, and management practices, which are the primary controls on SOM. The Inputs section then ends with erosional inputs, which relates to the new central objective identified in the response to Comment #1 from Reviewer 1, and a section related to SOM loss. The calibration and verification section, which relates to the inputs and SOM calculations, follows. Finally, the section ends with description of the model simulations for the calibration and verification simulations. The authors believe the Methods section now follows a more logical structure.

5a. Inside §2.3.4 on erosion (under the general header of §2.3 model input) starts on page 13 line 20 a description of the WEPP model. This description includes the

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rationale for applying WEPP on this site. So in this manuscript the model WEPP-output appears to be treated as an input to CENTURY (as also the USLE-output). This might be based on the chronology of the research.

The reviewer is correct to assume that WEPP and USLE erosion rates are used as inputs into CENTURY. WEPP was calibrated and validated for a separate project, which included the most current management practice; however, the calibrated model was used to simulate erosion rates for the earlier period. The description of WEPP has been shortened to address a comment from Reviewer 2. The section in the text still contains the reasoning for using WEPP.

5b. WEPP was most likely calibrated and applied firstly, later it was decided to combine this with CENTURY? However, this is not a logical structure.

Please see the previous comment. WEPP was calibrated and validated for a separate project, which included the most current management practice; however, the calibrated model was used to simulate erosion rates for the earlier period.

6. In §2.4 the model calibration and verification is described. This only deals with the calibration of CENTURY for the test field and is not mentioning WEPP.

The calibration and verification of WEPP are presented in separate manuscripts (listed below):

Papanicolaou, A. N., and Abaci, O. Upland erosion modeling in a semi-humid environment via the Water Erosion Prediction Project. *Journal of Irrigation and Drainage Engineering*, 134(6), 796-806, 2008.

Abaci, O., and Papanicolaou, A. N. 2009. Long-term effects of management practices on water-driven soil erosion in an intense agricultural sub-watershed: Monitoring and modeling, *Hydrological Processes*, in press.

The text in the section discussing WEPP was shortened and altered. References are provided for the WEPP descriptions, calibration and verification.

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"Detailed descriptions of WEPP are included in Flanagan and Nearing (1995), Renschler and Flanagan (2002), and Laflen et al., (2004). While calibration and verification of the model for the Clear Creek watershed are in Abaci and Papanicolaou (2009)."

Flanagan, D. C., and Nearing M. A.: USDA - Water Erosion Prediction Project: Hillslope profile and watershed model documentation, NSERL Report No. 10, West Lafayette, IN, 1995.

Laflen, J. M, Flanagan, D. C., and Engel, B. A.: Soil erosion and sediment yield prediction accuracy using WEPP, Journal of the American Water Resources Association, 40(2), 289-297, 2004.

Renschler, C.S., and Flanagan, D.C.: Implementing a process-based decision-support tool for natural resource management the GeoWEPP example. In: Rizzoli, A. E., and Jakeman, A. J. (Eds.), Integrated Assessment and Decision Support. IEMSS 2002: Interl Environl Modeling Software Soc., June 24–27, 2002, at University of Lugano, Switzerland, 3, 187–192, 2002.

7. Conclusion for section 2, "Material and methods", is that the structure of this part is poor and needs to be rewritten.

Please see the response to Comment #5 from Reviewer 1.

8. In section 3, "Results and discussion", it is stated that "The foci of this study were the changes in SOM dynamics resulting from shifts in different management practices and the effects of utilizing deposition rates in SOM evaluations." In this statement the spatial issues which were claimed in the introduction as one of the shortcomings in current knowledge is not present.

Please see the response to Comment #1 from Reviewer 1.

9. It appears that spatial issues are understood by the authors as different parts of the landscape and not as a spatial variability within the same unit. However, this is unclear.

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The spatial issues, to which the authors were referring, did correspond to different parts of the hillslope, namely the upland and floodplain; however, the text has been altered to clarify that the focus of this paper is the importance of deposition and not spatial variability.

10. Section 4 Conclusions. In this section a lot of general talk is given on the importance of the interaction SOM and erosion and a summary of the research. This is out of place. Only a few lines give real conclusions and even those are relatively trivial. We do not need two models to know that in the floodplain there is deposition, which carries SOM from the eroded soils originating from the upland.

The authors have reorganized the conclusion to provide more of a summary of the paper's goals and key findings. The general talk regarding the importance of SOM was shortened moved to the end. This section was kept to show this study in a broader perspective.

For SOM models that have erosion and deposition as only an input may require two models, as presented in this study. The models were not used to prove deposition occurred; however, the were used to provide quantifiable values.

10a. Example sentence out of the conclusions on pg 1601 line 13-16 states "To date, few studies have examined in detail the role of spatial and temporal variations of erosion/ deposition on SOM, i.e., the role of deposition is still poorly understood." So the spatial variations and now also the temporal one seems to be like a red thread throughout the manuscript.

Please see the response to Comment #1 from Reviewer 1.

10b. It is also odd to see that the very last sentence in the conclusion (page 1602 line 5- 7) contains a recommendation to use another model DAYCENT, which is not in the introductory literature review.

The statement referring to DAYCENT was removed from the text.

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Overall conclusion: this manuscript has a very poor structure. In its current form it is not acceptable.

The authors would like to thank the reviewer for his comments. The authors feel that the manuscript has been greatly improved by incorporating the reviewer's suggestions.

Anonymous Referee #2

1. Overall: The ultimate fate of soil eroded from agricultural uplands is a very important research topic, and it is difficult to study. Usually, all eroded material has been considered lost from the soil system, potentially skewing estimates of soil carbon budgets at field, regional, and global scales. This research has the potential to make an important contribution by linking field and modeling approaches in a single agricultural field, but the results are explained poorly and the conclusions are weak. The manuscript should be completely rewritten, with a focus on describing the main results and the specific implications of the results.

Please see the response to Comment #1 from Reviewer 1. The paper has been adjusted to focus on the results relating to the primary objective, which is to better understand the importance of deposition.

2. Specifics: Because the manuscript needs so much work, I cannot provide line edits. The same vague phrases are repeated throughout the manuscript, obscuring important concepts.

Please see the response to Comment #3 from Reviewer 1.

2a. For example, deposition is not defined clearly. This is a general word that could mean a lot of different things. What does it mean that deposition "muted" SOM loss due to erosion?

In the Introduction, deposition was defined as a component of "erosion" and the spatial redistribution of sediment. Deposition is the settling of sediment that is in transport. The term "muted" has been replaced in the revised manuscript with words such as

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“decreased” or “lessened”.

3. The methods section is much, much too long. Detail about USLE, WEPP, and CENTURY are widely available in the scientific literature.

The Methods section was shortened by removing much of the descriptions for USLE and WEPP. The removed sections have been replaced with only references. The section on the CENTURY description was kept intact because it provides explanation for terms and phrases in latter parts of the manuscript. To remove this section may lead to confusion for the reader.

4. The three modeling scenarios are set up well, but it is no surprise that dividing the field into an erosional upland and depositional floodplain worked the best. What is actually novel or surprising about these results? How did the study advance understanding of the consequences of erosion?

Reviewer 2 identified the importance of the paper in their first comment: The ultimate fate of soil eroded from agricultural uplands is a very important research topic, and it is difficult to study. Usually, all eroded material has been considered lost from the soil system, potentially skewing estimates of soil carbon budgets at field, regional, and global scales. This research has the potential to make an important contribution by linking field and modeling approaches in a single agricultural field.

This manuscript presents the potential error associated with considering all eroded material is lost from the soil system. In addition, this manuscript provides a means of addressing this concern.

5. The changes in SOC quality (Labile and recalcitrant are not explained clearly. Could it really be true that eroded material that is subsequently deposited is all light fraction? What about mineral bound organic matter?

The authors agree with Reviewer 2 that all deposited organic material does not consist of the light fraction. The text has been altered to reflect this and avoid further confusion:

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The deposited material would more easily decompose because it consists mainly of the light fraction (LF). Due to the low density of the LF, it will preferentially be entrained in runoff and transported downslope. The higher transported loads of the LF will lead to enhanced decomposition in the active layer of the floodplain.

6. The results raised several doubts about the model that were not adequately addressed.

6a) The monthly timestep of CENTURY is too long?

The present research was performed using CENTURY simulations that were limited to monthly predictions of SOM loss. This inherent limitation of the CENTURY model did not hinder understanding of the SOM resulting from different management practices. Changes in management practices occurred in periods longer than the monthly time step. Hence the model captured the effects of changing management practices on SOM dynamics. However, future studies that intend to capture daily changes in SOM due to different anthropogenic activities should consider the use daily event models like DAYCENT.

6b) The depth of soil considered with CENTURY is too shallow?

For total SOM budgets in the soil, the depth restriction in CENTURY does seem limited. However, the focus of CENTURY is on decomposition as a control for SOM concentrations and not erosion (or deposition). Decomposition strongly decreases with depth, so the depth of the model's active layer was not limited, in this respect. Decomposition is most prominent in the surface layer.

6c) Why was there no net increase in SOM?

The reviewer questions why SOM concentrations declined, if deposition was occurring. Firstly, at the hillslope scale more erosion occurred than deposition. There was a net loss of sediment and SOM from the field, which was mentioned in the text: Overall, the field has experienced a net loss of soil during the cropped period. On the floodplain,

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specifically, the decline also resulted from a model limitation, which was mentioned in the SOM Loss section 2.2.5. CENTURY focuses on only the active surface layer, whose depth is set by the user through the parameter, EDEPTH. As deposited sediments added depth to the active layer, the corresponding amount was removed from the bottom of the active layer to maintain the assigned depth, i.e., buried SOM was removed from the active layer. The deposited material, which was comprised mostly of the light fraction, was different from the SOM material removed (more stable forms of SOM). The deposited light fraction decomposed more easily. Thus, SOM concentrations in the active layer declined due to decomposition.

6d) Figure 5, showing the “spin-up period” is not necessary. Similarly Figure 1 does not add much and does not directly relate to the work described in the manuscript.

These figures have been removed from the revised manuscript.

The authors would like to thank the reviewer for his comments. The authors feel that the manuscript has been greatly improved by incorporating the reviewer’s suggestions.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 1581, 2009.

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