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Hydrology and Earth System Sciences Discussions

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

Anonymous Referee #1

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Review HESS-2009-34-discussion paper

Title SOM Dynamics and Erosion in an Agricultural Test Field of the Clear Creek, IA Watershed

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

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. A lot a statements are true but hold little or no information.

Example of such sentence (pag 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 ?

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.

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 deltat but with the ordinary "d" as in an ordinary differential equation. See later also equation 2 which suffers in the same way.

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.

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

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

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

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.

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.

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.

Example sentence out of the conclusions on pag 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.

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.

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

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

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Interactive Comment