

## ***Interactive comment on “A method to detect soil carbon degradation during soil erosion” by C. Alewell et al.***

**Anonymous Referee #3**

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The authors present an interesting study that addresses soil erosion induced degradation of soil organic matter. Based on a comparative study of the stable isotope composition of an eroding upland, a wetland with deposition and wetland without deposition, they conclude that significant degradation occurs during and after transport. Although I am excited about the use of new tools proposed here to understand erosion-carbon interactions, in my opinion, several issues need to be addressed before publication.

1. The approach is based on concentrations and not inventories. This is reflected in the absence of a depth component in equations 1-5. A soil profile under the impact of deposition is not a closed system where OC simply mixes. When comparing different sites, one needs to consider inventories at relevant depths and not simply concentrations and/or fixed depths. The authors state that the profiles  
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in the wetlands accumulate at a rate of 0.05-1.4 cm/yr (peat accumulation) and that there is an additional accumulation of ca 0.02 cm due to clastic deposition (based on a rate of 2 t/ha yr). This implies that the use of a constant observation depth (10 cm in this study) for both eroding, reference and depositional sites is not valid and that equations 1-5 are not fully describing the system. Given these high accumulation rates, only a few percent of the SOC<sub>input</sub> term (equation 3) will be retained in the upper 10 cm in this dynamic profile after several decades or centuries of deposition. It seems unlikely that changes in the isotopic composition of the bulk soil can be related to erosion induced processes, given its (very) small signal.

2. The method critically depends on the assumption that the isotopic composition of the deposited OC is the same as the composition from the source areas. As the authors point out themselves, much more OC is eroded (ca 30 t/ha yr vs only 2.6 deposition in the wetland). This seems like a realistic figure as it is widely accepted that less than 30% of the eroded sediments on first-order hillslopes are retained while the remaining 70% leaves the system (eg fluvial export or deposition in higher-order systems). As the first reviewer already pointed out, it is very likely that selective processes occur in these environments with a high surface roughness and which are dominated by sheet flow: the larger size classes and more dense fractions (eg mineral) are more likely to be trapped in these wetlands while a large fraction of the eroded material (smaller size fractions and lighter material) may be further transported. It is well documented that the isotopic composition of these fractions are very different. The interpretation of the isotopic data therefore requires a better quantification of the selectivity of the deposition process in order to obtain a robust estimate of the initial composition of the depositional wetland. In the absence of such information, the interpretation of the data remains difficult.
3. Secondly, the authors should provide quantitative evidence that the reference

site (C) has the same environmental conditions as the depositional wetland (B) in order to exclude differences in decomposition. The assumption of an identical environment is key to the interpretation of the results. Is it possible that the depositional wetlands are slightly more elevated than the reference profiles in the flat valley bottom and are not completely anoxic (at least during some periods of the year)? This could explain the observed differences in the isotopic composition.

4. Literature: The use of stable isotopes in erosion/deposition studies along a catena is relatively new but other studies have used it before. In my opinion, this should be properly referenced eg Wynn et al., *Geoderma*, 2006. In addition, there are a few publications about erosion-carbon interactions in wetland areas using  $^{137}\text{Cs}$  suggesting that no degradation takes places, but that net uptake of C occurs (see recent publications of Ritchie et al and McCarty et al). It would be interesting to discuss why the results obtained here are contradicting these earlier studies. In general, I believe that this paper could benefit from a better and more in-depth discussion of the results, especially in relation to other studies.
5.  $^{137}\text{Cs}$  technique: I believe the authors could further improve the use of the available  $^{137}\text{Cs}$  data. For example, only the wetland data is presented but the  $^{137}\text{Cs}$  activities at the eroding hillslope would give an indication of sediment and OC production. Furthermore, a detailed depth core in the wetland will give a deposition rate and a time interval for both Tchernobyl and 1963 (bomb-tests). This could be used to quantify the fraction of sediment and OC originating from the uplands, present in the upper 10 cm of the profile.

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