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Biogeosciences
Discussions

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

Anonymous Referee #2

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I have read this manuscript with great interest: from my research on the relationship between carbon dynamics and erosion I have learnt that we cannot understand erosion-carbon relationships by looking at carbon stocks alone. We need more tools and this paper is an exploration of the possible application of isotopic ratios in order to further understand what happens with carbon when eroded and (re-)deposited.

I agree with ref. 1 that reference to the previous ms could be made somewhat more explicit. However, I think the authors are indeed using the data in a novel and creative way and the ideas they promote certainly warrant publication. However, I do have some reservations with respect as to how the idea (to use isotopic ratios as an indicator for SOC degradation during transport) is worked out.

My main concern is with respect to the calculation of erosion/sedimentation rates from

137Cs data. The formula that is used (p5777 l 3) is based on measured concentrations of 137Cs. However, erosion and deposition rates can only be correctly be assessed by using inventories, rather than concentrations. The authors sample the upper 0.1 m of sediment: however, when 137Cs is deposited in a non-tilled environment, it is rapidly fixed to the sediment and most of it will be found in the upper 0.005-0.02 m of sediment (see papers by Walling and Quine in the 1980s and 1990s). That means that the concentration of 137Cs in the soil that is being eroded is much higher than the average concentration measured in a 0.1 m deep soil sample. A consequence of this is that there is a serious risk that erosion/deposition rates (what you call erosion rates are in fact deposition rates I think) can be strongly overestimated. I added a small spreadsheet calculation to illustrate this.

The only correct way to derive sedimentation rates is the establishment of a 137Cs inventory, i.e. you need to know how much 137Cs there is present per unit of surface area at a given landscape location. Even then, deposition rates cannot be unequivocally determined: I suspect that in the environment you describe erosion and deposition will be highly selective : this could be assessed by measuring the grain size distribution of the detritic material present in wetland B and to compare it to the grain size distribution of the eroded site (A). If both are more or less similar you may assume that there is a linear relationship between the excess 137Cs inventory found at the depositional site B and the deposition rate. If not you have to correct for the fact that 137Cs is preferentially bound to the clay fraction.

I strongly feel a correct estimation of deposition rates is a prerequisite in order to come to a final publication. I think the authors have the necessary data to calculate 137Cs inventories for all sites: these then should be used to calculate erosion and deposition rates according to procedures outlined by Walling, Quine and others.

After this I also think that the results should be more critically discussed. I do agree that the difference in isotope ratios between sites B and C is surprising and that the heavier signature at the B sites may be the result of the influence of sedimentation.

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The fact that the signature is much more heavy than expected from the input of eroded material is puzzling and might be due to degradation during transport (but I will come back to that) but there is a perfectly plausible alternative. As you indicate yourself, total carbon content at the B-sites is much lower. At present it is not clear from the MS how much lower (I think), but looking at Fig. 8 of the RCMS MS it seems that carbon contens on the B-sites may be only half of that on the C-sites. This difference cannot be explained by deposition effects only: you would need really high deposition rates to achieve this. It is much more likely that slight differences in hydrologic regime and primary production, perhaps indirectly caused by deposition, are responsible for this difference, but it is hard to know this for sure. In any case, the lower carbon contents at the B-sites appear to me as an indication that there is more carbon mineralization at the B-sites than at the C-sites. This will almost certainly lead towards a higher deltaC-13 ratio due to fractionation.

Another issue that needs to be discussed is the degree of fractionation that would be needed in the degradation process in the transport phase if one assumes that degradation during transport is indeed the key process. I made a quick calculation (also attached) and if I have got it right you would have to shift the deltaC13 to -15 0/00 (from -27 0/00) if one assumes erosion during 50 years etc..Now, that is a really big shift it seems to me: is this possible ? Is there any literature that supports the existennce of degradation processes that cause such a rapid shift ?

I would therefore suggest that the authors take a close look again at their data and use them to calculate deposition rates as accurately as possible: this may result in both a higher or a lower number than is currently proposed. In fact deposition rates in wetlands can be quite high and this may also explain the larger-than-expected shift. Next, I would propose the authors would discuss their idea more critically taking into account the remarks formulated above and taking care in using correct terminology (erosion vs. sedimentation).

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Please also note the Supplement to this comment.

Interactive comment on Biogeosciences Discuss., 6, 5771, 2009.