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Interactive comment on "The effect of soil redistribution on soil organic carbon: an experimental study" *by* H. Van Hemelryck et al.

Anonymous Referee #3

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The effects on the carbon budget of redistribution of soil organic carbon as a result of soil erosion are highly disputed in the international literature. Erosion and in particular changes in erosion rates and processes, can lead to mineralization of displaced SOC. The authors of this paper present an interesting data set dealing with this hot topic of Biogeoscience. They quantified redistribution of soil from an erosion flume to a deposition area and the associated changes in mineralization of the deposited C. Such experimental data are extremely rare in the international literature. Despite the strong need to have such studies I have some reservation to recommend publishing of this study because of some methodological shortcomings. To my opinion the main weaknesses are:

1. The largest differences in C mineralization occurred between the WSR and DSR

treatment where the authors used re-wetted and air-dried soil, respectively. It is well known that re-wetting a dry soil causes a (large?) pulse of C mineralization. In the moistened treatment (WSR), this C mineralization occurred before initiating the erosion event and was not measured. In contrast, in the DSR treatment the re-wetting pulse occurred just after starting the erosion event and was part of the measurements. The authors did not give any information about the time between re-wetting the soil and starting of the erosion event (WSR treatment). Also in the MR treatment C mineralization as a result of preparation of the soil suspension was not measured. It might be that the larger C mineralization of the DSR treatment in comparison to the other two ones is just the result of missing C mineralization after rewetting a dry soil in the WSR and MR treatments. It should be avoided that one of the main conclusions ("deposition of eroded sediment indeed led to a significant additional CO2-efflux towards the atmosphere during the DSR experiments") are the result of an experimental artifact. 2. The control treatment is not clear for me. Was this treatment (depositional area) also exposed to water flow? I would expect some effects on C mineralization just by applying some water. At least it should be clarified by the authors. 3. I did not understand the rationale to change the soil incubation after 77 days and to move the soil columns from the lab into the field. To my opinion the variability increased (temperature, humidity and wind...?) and I do not see an advantage in comparison to a continuation of the incubation study in the lab. It is a lab study and the character of the study did not change just by doing a part of the incubation outside. 4. The extrapolation of C mineralization of the MR treatment is not convincing. In the first days, mineralization is normally largest. Furthermore, the temperature was highest just after bringing the soil columns from the MR treatment into the lab. Therefore, I would assume that mineralization of the MR treatment was overestimated by this approach. A large impulse of CO2 was observed after moving the samples from the lab into the field. This pulse might be affected by the previous length of the incubation period in the lab. A larger pulse might occur if incubation in the lab was shorter because less amounts of labile C were already respired. Therefore, mineralization data of the MR treatment are not very well comparable to the other ones. 5. I would appreciate to get some information about aggregation of the soil, organic matter associated with aggregates and how this was affected by the treatments. It is an important point of the argumentation, however, without any data. Probably the authors have some other data (organic C distribution in particle sizes and / or density fractions) usable for discussion of the data. On the other hand, the character of deposited OM was not changed by the different treatments – an argument that the degree of disaggregation did not differ between the treatments.

I have some other comments as well:

Abstract CO_2-measurements were not done on undisturbed soils neither at field conditions. I do not understand where the numbers 14-22% are coming from. It was also written in the text that between 14 and 22% of eroded SOC was mineralized (page 5054, line 18). At the same page the authors stated that the additional CO_2 efflux was between 2 and 12%. That is not clear and should be explained / revised throughout the entire manuscript.

Introduction Although the introduction seems to be a little bit too long it gives a comprehensive summary of the current literature. Also the research objectives are convincing. However, no hypotheses were given.

Statistics At page 5042, lines 16-18 ("The measurements on the undisturbed soil cores, sampled on similar locations in the depositional area after the two experimental runs, were considered as replicates.") it is postulated that several cores per treatment and position were sampled. However, the number of replicated cores was not given. I am not sure if such replicated soil core sampling was possible taken the dimension of the depositional area into account. I would assume that the number of replicates were 2. That should be clearly stated; also in the legends of the figures. Might it be possible to use the deposition zones as co-variables?

Results Page 5050, line 12: again some clarification needed regarding to the numbers The authors gave 4, 12 and 2 % of total C. However, in Table 6 4, 12 and 4 % were given.

Discussion C mineralization might be similar because of the same source of the used SOM. Page 5052, line 27-29: experimental artifact (see above)?? The aggregate size and the distribution of OM to these aggregates of the various treatments (inlet and outlet) might be interesting and helpful for the discussion (see above).

Conclusions Most parts of the conclusions are just a repetition of the summary.

Tables Table 3 and 4 can be omitted. That also means that the respective paragraphs in the text of the manuscript can be shortened.

Figures Figs. 3 and 4 are too small. The dots and lines represent soil moisture should be mentioned in the legend of fig. 4. Soil moisture measurements were stopped in the second treatment, why? The indication of M1 and M2 in the legend of fig. 5 might be misleading.

I think that some of my reservations might be fixed by revision of the paper. However, I got the feeling that the quality of the paper can be greatly improved by some additional measurements, e.g. quantifying C mineralization as a result of re-wetting. This is little work in relation to the big efforts of all the work already done and the potential large impact of this study.

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