**Biogeosciences
Discussions**

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

**H. Van Hemelryck et al.**

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We would like to thank all anonymous referees for their valuable comments on the content of our manuscript and their suggestions for improving the document. In this reply, we seek to clarify the issues, raised by the referees, point by point. Similar questions by multiple referees are grouped accordingly.

1) As noted by two referees, a major limitation of our study is the lack of quantitative data about soil aggregation and aggregate-associated C (referee
2, comment 1; referee # 3, comment 5):

Our main goal was to investigate the effect of different types of erosion events on SOC dynamics, thereby using a set-up that, unlike any previous studies, allows mea-

surements in conditions closely resembling a field situation (*in situ* measurements of $CO_2$-efflux on undisturbed soil instead of incubating runoff or extracted soil samples). The main conclusions, drawn in our manuscript, are based on the significant prolonged differences in $CO_2$-exchange between the deposited soil and the atmosphere while environmental factors (temperature, moisture, . . .), controlling $CO_2$-efflux, remained constant. They show that the *in situ* measurement of C dynamics after an erosion event leads to conclusions that are indeed significantly different from incubation experiments. We are convinced that our results do support our main conclusions that (i) the type of erosion event plays a role in subsequent C mineralization and (ii) in situ measurements provide information that allows to account for mechanisms not accounted for by incubation measurements. We do indeed hypothesize that the observed differences are related to differences in aggregate breakdown and that the latter are dependent on initial soil moisture content, an assertion that is supported by a vast body of literature (e.g. Le Bissonnais (1996)). Our current study does not allow us to quantitatively assess the relationship between aggregate breakdown and C dynamics, but this was not the main objective of the study. Rather we wanted to investigate if the type of erosion event had significant effects on C dynamics and to see if investigating this relationship warranted further effort and we therefore agree with ref.#2 and ref.#3 that additional information about the changes in aggregate structure of the soil as well as data on the relative distribution of aggregate-associated carbon fractions, are most welcome in follow-up experiments to firmly interpret the findings of this study.
We include here the results of some additional measurements on the impacts of wetting on aggregate structure and carbon distribution for the soils used in this study. The methodology used for this analysis was adapted from Elliott (1986). Briefly, two sets of three 50-g subsamples were taken from the original soil, after this was passed through a large meshed sieve, air dried and stored at room temperature. Aggregate separation of these samples was done by wet sieving. However, for the two sample sets, a different pretreatment was applied: (i) for the first set the air-dried soil was rapidly immersed in water (initiating slaking) while (ii) for the second set the air-dried soil was slowly wet-

ted to field capacity before immersion in water (to a soil moisture content similar as in WSR). As such, the wet sieving treatment resembled the erosion process for the DSR (i) and the WSR (ii) in the original experiments. Next, the soil samples were submerged in water on a 2000 $\mu$m sieve for 5 min. Subsequently the soils were sieved under water by gently moving the sieve up and down 3 cm for 50 times in 2 minutes. Soil material remaining on the sieve was oven-dried and weighed. The material < 2000 $\mu$m was collected and the sieving procedure was repeated for sieve sizes of 250 $\mu$m and 53 $\mu$m, thereby separating the original soil samples in 4 aggregate size fractions. Total organic C content of each aggregate fraction was determined by high temperature combustion using a Variomax CN analyzer.

As expected, rapid wetting resulted in more important aggregate breakdown: in the slowly wetted soil samples, macro-aggregates (> 250 $\mu$m) constitute 52% of the dry soil weigh compared to 39% for the rapidly wetted soil samples. The slaking treatment resulted mainly in a strong reduction of large macro-aggregates (> 2000 $\mu$m, 13% compared to 3%), concomitant with an increase in micro-aggregates, while the proportion of silt and clay size particles only increased slightly (Figure 1). Organic C concentrations were rather similar among aggregate size fractions > 53 $\mu$m in the slowly wetted soil, while SOC concentration decreased with decreasing aggregate size in the slaked soil samples (Figure 2). These results are supportive of the aggregate hierarchy theory of Tisdall and Oades (1982): in comparison to slow wetting, slaking leads to to a stronger disruption of easily degradable macro-aggregates into much more stable microaggregates. Evidently, this will lead to the exposure of the carbon-rich cementing material binding the macro-aggregates (microbial and plant-derived polysaccharides, roots and fungal hyphae) thereby increasing the potential for mineralization during the erosion/transport/deposition process. These experimental findings support the hypothesis put forward in our paper in order to explain higher SOC mineralization rates after DSR experiments: the more intense destruction of macro-aggregates during rapid wetting may indeed explain higher carbon mineralization rates in the period following the erosion event.

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However, further studies are clearly necessary to confirm this hypothesis.

2) Related to comment 1), referee # 2 denounces the fact that, within this study, it is inherently assumed that carbon, protected in soil aggregates, is labile carbon, albeit this assumption is not supported by quantitative data on aggregate-associated C (referee # 2, comment 2).

As already stated, our main goal was not to quantify aggregate-carbon interactions. In the first place we wanted to investigate which factors controlled C mineralization if a setup, mimicking field conditions, is used rather than incubation. Clearly, having found differences between runs that are most likely to be attributed to differences in aggregate structure and aggregate-associated C, it is recommended that in follow-up experiments this question is more explicitly addressed.

3) Referee #2 and Referee #3 signaled the possible misconception regarding the setup and the function of the control treatment in this study. This mainly resulted from an unclear and/or too concise description in the manuscript. (referee # 2, comment 4; referee # 3, comment 2).

We have changed the description in the paper to make the role of the control experiments clearer. A detailed explanation of the modifications follows below.
In this study, eight soil beds were similarly prepared in identical trays (2.25 m x 0.61 m x 0.25 m) (see paragraph 2.1). Six soil beds were used as a depositional area during the experiments, two as a control soil. In the manuscript, this is described as [paragraph 2.1, p 5036, line number 18-19]:"In total, six depositional areas were prepared for the experimental runs and two as additional soil beds". We changed the above to "In total eight soil beds were prepared in identical soil trays, six as depositional areas for the experimental runs and two as control soil beds".
After the experimental runs, three undisturbed soil cores were sampled in each soil bed by inserting an inner and outer PVC ring of 0.2 and 0.3 m respectively (see paragraph 2.2). In the control soil beds, three undisturbed soil cores were similarly sampled and

further termed 'control soil cores'. These control soil beds were not exposed to water flow and no sediment was deposited onto them. We changed [paragraph 2.2, p 5038, line number 21-22] to "Using an identical procedure, three undisturbed soil cores were sampled from the control soil beds (further termed 'control soil(s) cores'), which were prepared similarly to the depositional soil beds but were not exposed to water flow and onto which no sediment was deposited.".

After the experimental runs, temperature, moisture and $CO_2$-efflux, were measured on all sampled soil cores, including the 'control soils' (see paragraph 2.5). We changed [paragraph 2.5, p 5041, line 8-10] to "Additionally, measurements were carried out on the 'control soil cores', which were sampled from the control soil beds onto which no sediment was deposited. These measurements were used as a reference to which $CO_2$-efflux from soil cores, sampled in soil beds with deposited sediment was compared.".

4) Referee #3 remarks that the use of different numbers (14 – 22 % and 2 – 12 %) with regard to effect of erosion/deposition on $CO_2$-efflux is confusing. (referee # 3, additional comments)

In paragraph 3.2, a comparison is made between the mass of SOC in the deposited sediments, before, and after a 98-day period of $CO_2$-efflux measurements. The difference in SOC between the end and the beginning of this period, expressed relative to the mass of initially deposited SOC, amounts to 20% (for WSR), 14% (for DSR) and 22 % (for MR). These numbers can be considered as the fraction of deposited SOC, respired during the measurement period. It would, however, be erroneous to interpret these percentages as indicative for the net impact of erosion/deposition on SOC during each experimental run.

The deposited sediments cap the soil bed in the depositional area and thereby possibly suppress the initial $CO_2$-efflux from this soil bed (without the overlying layer of deposited material). This initial efflux could be presumed to equal the $CO_2$-efflux, measured on the, similarly prepared, control soil bed (see point 2). A more precise

measure for the net impact of erosion/deposition on SOC could therefore be calculated as the amount of respired $CO_2$-C additional to the cumulative $CO_2$-efflux from the control soils. This difference should then be expressed relative to the amount of deposited SOC (e.g. as done in Polyakov and Lal (2004)). For WSR, DSR and MR, this calculation yields 4%, 12% and 2%, respectively. In Table 6, the latter numbers are incorrectly reported as 4%, 12% and 4%.

In order to remove confusion about the meaning of these numbers, we:
- no longer mention "between 14% and 22%" [abstract, p 5032, line number 16-17] in the abstract of the manuscript as the interpretation of these numbers in this context requires a more substantial discussion about their calculation.
- have changed [paragraph 3.2, p 5045, line number 19-21] to "The mass of SOC in the deposited sediments, immediately after the experimental runs, was calculated by method M1. After 98 days of $CO_2$-efflux measurements, this amount of SOC had decreased by 20% and 14% for the WSR and DSR respectively, as calculated by M2 (Table 2)."
- have changed [paragraph 3.4, p 5049, line number 8-15] to "For the 98-day measurement period (inside and outside the laboratory), a cumulative amount of 24.2, 44.6 and 23.9 g $CO_2$-C m$^{-2}$ was respired from the WSR, DSR and MR depositional area respectively. The cumulative respiration from the control soils during the same period amounted to 21.0 g $CO_2$-C m$^{-2}$. For the whole depositional tray, the additional emission of $CO_2$-C, relative to a control soil (and thus due to erosion/deposition) and expressed as a fraction of the mass of initially deposited SOC, amounted to 4 %, 12% and 2% for WSR, DSR and MR respectively. These values are considerably lower than the estimated decrease of SOC in the deposited sediment, previously calculated as the difference in mass of deposited SOC before and after the $CO_2$-efflux measurements."

5) According to referee #1 and referee #2, in order to draw valid conclusions from the $CO_2$-efflux measurements, the mass of respired $CO_2$-C should be expressed relative to the mass of deposited sediment or SOC, instead of per area and time (g $CO_2$-C m$^{-2}$

s$^{-1}$), as is done in the manuscript. (referee # 2, comment 7; referee # 1)

Given the different amounts of sediment and SOC, deposited during each experimental run, this seems to be a logical line of argument. In similar research papers (Jacinthe et al., 2002;Jacinthe et al., 2004), measurements of $CO_2$-efflux from incubated runoff sediment were presented accordingly and we already did this to some extent in the original MS in [paragraph 3.2, p 5045, line number 19-21]: the decrease of SOC in the deposited sediments after $CO_2$-efflux measurements is expressed relative to the initial SOC content (see also comment 4).

However, interpreting the results as such leads to rather ambiguous conclusions. During the experiments, the sediment deposits were formed on top of a 0.22 m thick soil layer. Thus, the $CO_2$-efflux was measured on a soil column consisting of 0.22 m of the original soil bed below a layer of eroded and deposited soil material, variable in depth. The $CO_2$ leaving the soil column may therefore come from the SOC in the sediment deposits as well as from the SOC in the original soil below. However, when comparing total SOC mineralization (cumulative $CO_2$-efflux) from the soil columns with deposited sediment, with the, independently measured, decrease in SOC in these deposited sediments, we found similar values. The latter indicates that the deposited layer was indeed the most important locus of SOC mineralization. Importantly, this implies that SOC mineralization within the original soil was significantly slowed down by the presence of a deposited layer.

We also noted that rates of $CO_2$-efflux, did not significantly vary with the thickness of the deposited layer (for a given type of experiment). This implies that not all SOC, present in the deposited layer, is mineralizing at the same rate, making it impossible to use the total amount of SOC that is deposited as a reference value.

Therefore, we chose to follow Polyakov et al. (2004), who, for a similar set-up, although on a smaller scale, also expressed $CO_2$-efflux per unit area. The net impact of erosion/deposition on SOC mineralization was then calculated as the amount of respired $CO_2$-C additional to the cumulative $CO_2$-efflux from the control soils (as explained in comment 4).

6) Referee #3 notes that the observed difference in $CO_2$-efflux between WSR and DSR, instead of resulting from aggregate disruption, might equally well, be an unaccounted for effect of different rewetting times of the initially air-dry soil. (referee # 3, comment 1)

This is indeed a valuable hypothesis and we therefore included a short discussion on this issue in the MS. However, we do not believe this phenomenon can fully explain the differences observed, as the effects of such wetting pulses should be short-lived (the wetting was carried out one day before the experiment) and differences in $CO_2$-efflux were measured up to 77 days after the experiment.

7) Referee #1 is puzzled by the unexpected increase of $CO_2$-efflux on all soil cores towards the end of the experiment. In case soil temperature is the driving force behind this increase, its effect could then, in fact, be supposed to prevail over, or dwarf any effect of soil redistribution in a field situation (referee # 1, comment 4).

We included the paragraph below in the discussion to clarify our viewpoint:
From day 60 on, a temperature rise in the laboratory building (despite temperature control) due to sunny spring weather, led to a similar increase of soil temperatures on all soil cores. Concurrent with this observation an increase of $CO_2$-efflux is observed for all cores. Given the fact that this increase of $CO_2$-efflux and soil temperature was also observed for the control soil cores, the most probable reason of this observation is therefore the higher soil temperature. However, we also observed that the increased temperatures induced drying and cracking of the soil surface, possibly causing the release of $CO_2$, that was previously trapped within the soil pore space, to the atmosphere. This would then explain why effluxes decreased again from day 78 onwards, despite continued high temperatures.

8) After 77 days of $CO_2$-efflux measurements, the soil columns were moved from the lab to an outside environment, for another 20 days. Referee # 2 and referee # 3 contest the added value of continuing measurements under variable external conditions and

suggest to elucidate the rationale behind this decision (referee # 3, comment 3; referee # 2, comment 5).

We added the paragraph below to clarify this decision.
After 77 days, no noticeable changes occurred in $CO_2$-efflux between the soil columns. A further continuation of the measurements was therefore not considered to be useful. We thereupon moved the soil columns to an outside location, where they were protected from rainfall, so that additional information on the effects of drying and temperature variation on $CO_2$-efflux could be collected. This phase of the experiment was considered to be exploratory only and was therefore limited to 20 days. The measurements in the outside environment, however, did allow to postulate hypotheses, which could inspire discussion on this topic and direct possible further research.

9) Referee # 2: "Under what conditions (temperature, water content) where $CO_2$-efflux measurements carried out and was this identical for all samples?" (referee # 2, comment 5)

This is explained in the manuscript.
Soil temperature and soil moisture content were kept constant (except for a stabilizing period, immediately after conducting the experiments) and identical for all soil cores ([paragraph 3.3, p 5046, line numbers 7-10] and [paragraph 3.3, p 5047, line numbers 6-8]) during the first 56 days of the measurements. This is also the period on which the statistical analysis of $CO_2$-efflux, measured inside the lab, was based.
60 days after the start of the measurements, an increase in solar insolation and outside temperature led to a similar warming of all soil cores by 2.8 °C ([paragraph 3.3, p 5046, line numbers 10-11], comment 7). Soil moisture decreased slightly but similarly for all soil cores in this period.
After 77 days of $CO_2$-efflux measurements, the soil cores were moved to an outside environment (see also comments 7 and 8). During 21 days, soil temperature was variable in time, depending on the outside air temperature, with slight differences (0.5 °C – 1.0 °C) between soil cores from different experiments [paragraph 3.3, p 5046, line

number 28 and p 5047 line numbers 1 - 5]. Soil moisture content decreased steadily on all soil cores but at a quicker rate for the soil cores from the MR experiments [paragraph 3.3, p 5047, line numbers 11 - 15].

10) Referee # 1 supposes that, as a result of the sample preparation (moderate drying procedure), a considerable amount of organic carbon (DOC and POC) in runoff samples mineralized unnoticed during the drying of the samples. This would result in an underestimation of C export and thus incorrect SOC budgets (referee # 1, comment 2)

While it is clearly true that drying may, as such, have an effect on SOC mineralization, it is an inevitable step in soil preparation. We therefore carried out an additional test to assess the effect of the sample preparation: we did not find any signifant effect of the drying procedure and reported this in the manuscript.
An air-dry sample of the soil used in the experiment, was split up in 9 subsamples (3 treatments x 3 replicates). A first set of samples was moistened, similar to the soil bed during the WSR, a second set of samples was dispersed in water and a third set of samples was left dry. Subsequently, the first and second sets of samples were dried as described in the manuscript. Afterwards the SOC-content for all soil samples was determined. No significant differences could be detected between any of the sample sets.
This led us to conclude that the sample preparation method did not induce any serious biases. The useful remark of referee # 1 should, however, be taken into account in the elaboration of further research.

11) An increase of soil temperature at the start of the $CO_2$-efflux measurements on the MR soil columns induces some reservation with regard to the interpretation, extrapolation and comparison of the results of these measurements. According to referee # 3, the above is not sufficiently accounted for in the manuscript (referee #3, comment 4).

For practical reasons, the MR experimental runs were done at a later point in time than the WSR and DSR experimental runs [paragraph 2.5, p 5041, line numbers 11

- 14] and the $CO_2$-efflux measurements on MR soil columns were conducted during a smaller time period. In order to compare the cumulatively evolved $CO_2$-C between experimental runs, the average $CO_2$-efflux from MR soil cores, measured during the period in which the soil columns resided inside the laboratory, was extrapolated backwards for the period during which measurements were done on the WSR and DSR soil columns only [paragraph 2.5, p 5041 line number 28 and p 5042, line numbers 1 - 4]. Unfortunately, the start of the $CO_2$-efflux measurements on MR soil cores coincided with an increase of air and soil temperature (see comment 7). Because of the influence temperature has on $CO_2$-efflux, it is not be possible to correctly compare WSR and DSR measurements with MR measurements. The latter were therefore not included in the statistical analysis ([paragraph 3.3, p 5046, line numbers 11 - 16] and [paragraph 3.4, p 5048, line numbers 17 - 18]).

Cumulative $CO_2$-efflux and any thereof derived values, however, were reported as such in the manuscript (mainly [paragraph 3.4, p 5049, line numbers 5 - 13]). As correctly pointed out by referee # 3, these values might be overestimated for the MR experimental runs, due to the extrapolation.

A possible solution to this problem might be, to correct the extrapolation for the temperature increase, using temperature - $CO_2$-efflux relations, reported in the literature (although at the risk of inducing greater uncertainty).

We included a small paragraph after [paragraph 3.4, p 5049, line number 17], in which we explicitly state that the cumulative $CO_2$-efflux from MR soil columns might be somewhat overestimated, as compared to cumulative $CO_2$-efflux from WSR, DSR or control soil columns. It is worthwhile to note however, that the above does not not change (even on the contrary) our general conclusion that the $CO_2$-efflux from MR soil columns was unexpectedly small [paragraph 4.2, p 5053, line numbers 8 -13].

12) After comparing the range of $CO_2$-effluxes, measured in this study, with field measurements of $CO_2$-efflux, we stated ([paragraph 4.2, p 5051, line number 27 and p 5052, line numbers 1 - 4]): "Our laboratory results yield realistic values of $CO_2$-efflux and we may therefore have some confidence that our results are applicable to field

conditions". According to referee # 2, the above statement is a bit far-reaching, as it is not proper practice to compare the results of this study with those of other studies carried out under different climatic conditions and on different soil types. (referee # 2, comment 7)

We have modified this statement simply stating that "values of $CO_2$-efflux appear to be realistic when compared to those of field studies".

Our approach (measuring on undisturbed soil columns, yet under controllable conditions) yielded measurements in the same magnitude of $CO_2$-efflux, measured in field situations in general (e.g. as reported by Raich and Potter (1995) for a summary of studies in a wide range of conditions). This is unlike other studies, where measurements were done on runoff samples or very small soil sections, and where results were extrapolated to field scales or used as parameters in large-scale carbon-erosion models.

13) Referee # 2: What was the reason for the choice of the slope and the length of the erosion flume? (referee # 2, comment 3)

The experimental set-up, used in this study, was largely based on the set-up used by Beuselinck et al. (1999). The extensive work of himself and others (eg. Steegen et al. (2000)) on sediment entrainment, transport and deposition, conducted at the same research group for many years, provided the necessary knowledge to correctly set up the erosion flume and deposition tray (slope, length, water discharge) as to obtain the desired erosion rates and deposition characteristics.

14) Referee # 1: "Why is the sediment C concentration in the DSR outflow that high?" (referee # 1, comment 3)

Over all, little soil material was exported from the depositional tray (on average a 20- to 30-fold less than sediment inflow during the WSR and DSR). This small amount of exported soil was enriched in SOC, compared to the original soil material.

The enrichment was much more pronounced during the DSR ($ER_{SOC}$ of up to 6). We

hypothesize that the high concentrations of SOC in exported soil during the DSR are the result of the intense breakdown of soil aggregates during erosion and transport, resulting in the release of free-floating particles of organic-rich material and/or lightweight complexes of organic compounds and sediment particles.

The latter view is expressed in [paragraph 4.1, p 5051, line numbers 9 -13], although due to the lack of measurements of the nature of the exported SOC, the above hypotheses could not be proven in this study (see also comment and 1 and 2).

15) Referee # 3: "The number of replicated cores was not given, I would assume that the number of replicates were 2. That should be clearly stated." (referee # 3, additional comment)

The number of replicates indeed is 2. We will stress this in a revised version of the manuscript as suggested by referee # 3.

16) Table 3 and table can be omitted and the respective paragraphs in the text can be shortened (referee # 2, comment 10; referee # 3, additional comment).

We agree with referee # 2 and referee # 3 on the above and will remove the tables and respective text in a revised version of the manuscript.

17) Referee # 2 and referee # 3 consider figures 3 and/or 4 too small and/or of bad quality (referee # 2, comment 9; referee # 3, additional comment).

We slit figs 3 and 4 into 3 subfigures in order to increase legibility.
We dit not replace fig 3 with a table as suggested by referee # 2 The figure contains a time dimension which we deem important in understanding the evolution of sediment in- and outflow and its associated SOC. This subsequently allows to interpret the nature of the deposited material. In our opinion, a summarizing table would therefore involve a loss of information to the reader.

18) Referee # 3: "soil moisture measurements were stopped in the second treatment, why ?" (referee # 3; additional comment)

The soil moisture measurements were not stopped. As a result of a malfunction of the soil moisture measurement device, measurements taken during this short period turned out to be incorrect and were left out of the manuscript. We have indicated this clearly in a revised version of the manuscript.

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Tisdall, J. M., and Oades, J. M.: Organic-Matter And Water-Stable Aggregates In Soils, Journal Of Soil Science, 33, 141-163, 1982.
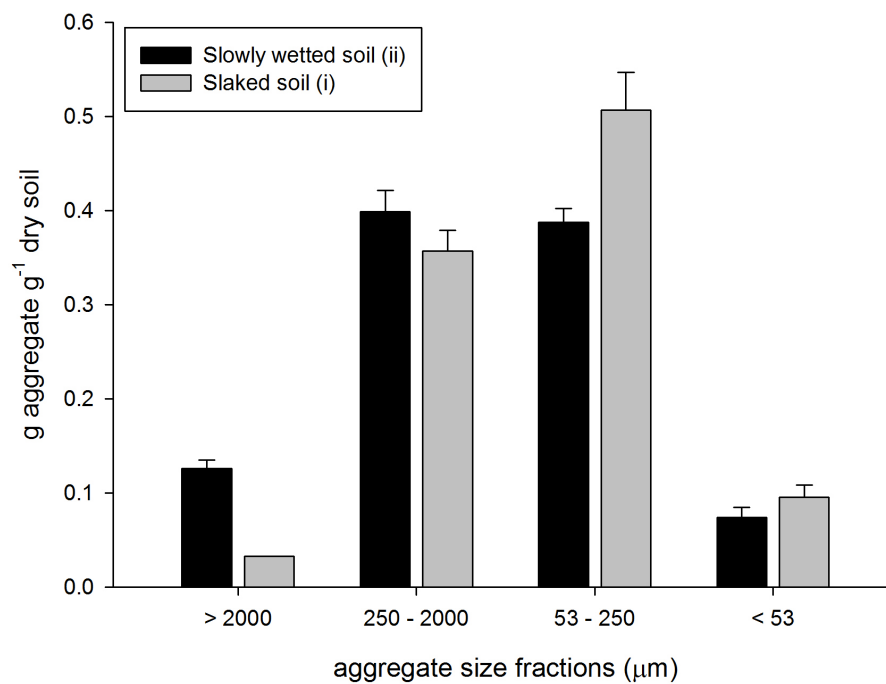
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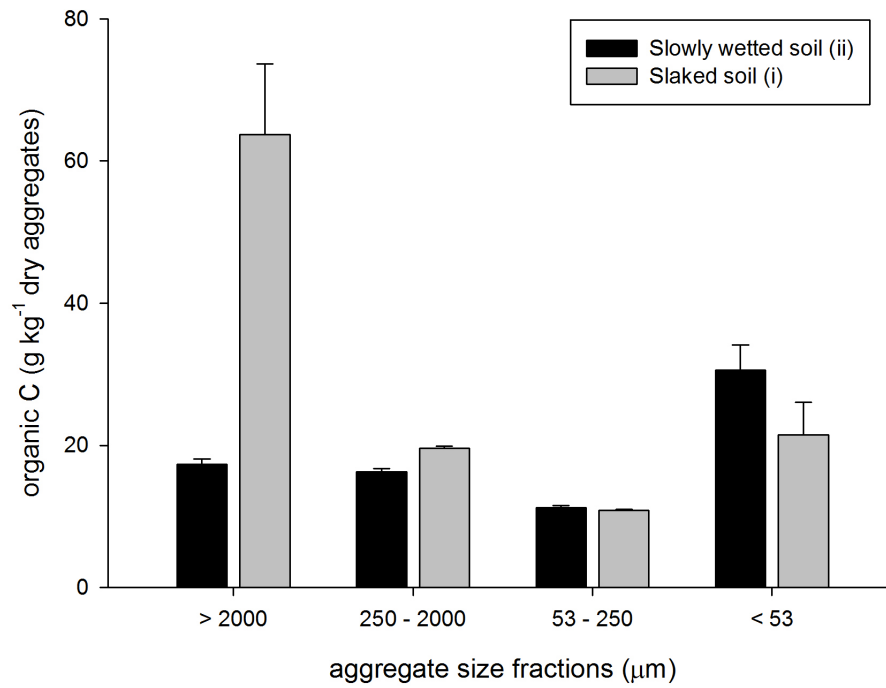**Fig. 1.** Aggregate-size distribution for slaked and slowly wetted soil.

**Fig. 2.** Organic C concentration for the aggregate-size classes for the slaked and slowly wetted soil.

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