Biogeosciences Discuss., 6, C1027–C1029, 2009
www.biogeosciences-discuss.net/6/C1027/2009/

Biogeosciences
Discussions

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

Anonymous Referee #2

Received and published: 6 July 2009

The authors quantified carbon exchange between the soi land the atmosphere during a laboratory experiment simulating three different types of erosional events. The decomposability of eroded carbon is an important subject concerning the soil carbon cycle, which has up to now rarely been addressed with experimental studies. Therefore the study is an important contribution to the erosion literature, which could be published of the following recommendations, were taken into account :

1) the main pitfall of the paper is, that the authors suggest, that the main process leading to carbon mineralisation after erosion is aggregate disruption. However, they never carried out measurements on carbon protected by soil aggregates and therefore have no quantitative information on this. Maybe it could be possible to assess the amount of carbon which can be liberated after slaking of wet and dry soil. This would

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

add great value to the paper. At least, the amount of soil carbon associated with aggregates should be quantified.

2) Moreover, they assume, that carbon protected in soil aggregates is labile carbon easily accessible to the soil microbial biomass after desaggregation. However, this is not necessarily be the case.

3) Material and Methods : what was the reason for the choice of the slope (15° as far as I understood) and the length of the erosion flume ?

4) Line 281 : why were undisturbed soil cores used as a control ? Where were they sampled ? The control for CO2 measurements should be the dry or wet soil which constituted the erosion fume.

5) CO2-measurements : under what conditions were the CO2 measurements carried out (temp., water content) was this identical for all samples ? As carbon mineralisation is most likely influenced by the eroded carbon type an dits degree of stabilisation, the other conditions (temp. and water content of the incubated samples) should have been identical to allow for comparaison, otherwise there is no point to carry out such measurements in the laboratory. What was the rational for measuring CO2 flux evolution after carrying the soil to the outside and its exposition to drying ?

6) Results : in my oppinion the paragraph 3.3 should be moved to the material and method section.

7) CO2 efflux data should be given in mg CO2-C per g soil C to account for different C content of the eroded sediments. This is particularly important for comparison of the three different treatments and for the discussion (line 595-606) where the authors compare their measurements with other studies concluding that their results are applicable to field conditions. In my oppinion, this conclusion is a bit far reaching as the other studies were carried out under different climatic conditions and most probably different soil types (no information on this was given).

BGD

6, C1027–C1029, 2009

Interactive Comment

8) Discussion line 647 : it should be added, that for a given site and soil type, the effect of erosion on SOC mineralisation depends on the type of erosion event and soil conditions.

9) Fig. 3 is of very bad quality. Average carbon concentrations of the original soil should be reported in a table maybe together with C average carbon content in sediments and enrichment ratios.

10) Table 3 and Table 4 do not contribute much to the main message of the paper and therefore should be deleted.

Interactive comment on Biogeosciences Discuss., 6, 5031, 2009.

BGD

6, C1027–C1029, 2009

Interactive Comment