Biogeosciences Discuss., 11, C3309–C3312, 2014
www.biogeosciences-discuss.net/11/C3309/2014/

Biogeosciences
Discussions
Open Access

Interactive comment on "Aggregates reduce transport distance of soil organic carbon: are our balances correct?" *by* Y. Hu and N. J. Kuhn

Anonymous Referee #4

Received and published: 7 July 2014

Comments on the manuscript "Aggregates reduce transport distance of soil organic carbon: are our balances correct?" (bg-2014-156)

The manuscript covers an interesting topic in that it tries to identify the role of soil aggregates on preferential carbon transport by erosional runoff events. In this regard it is considering a very interesting, controversy and actual debate about fate of SOC during erosional driven particle transport. As the authors correctly stated out, this controversy arises due to a knowledge gap about travel distance of detached soil particles and particle attached SOC. The authors try to fill this gap with measured data based on a combined approach of artificial rainfall simulation, size separation of transported particles via settling tube and measurements of SOC as well as CO2 respiration rates in different sized soil separates. They found that soil samples taken from rainfall experi-

ment show a clearly different particle size distribution than the original soil. In erosion exposed soil samples amount of particles/aggregates >63μm is 43% whilst only 8.9% in original soil. Moreover SOC in that particles/aggregate size differs from those without erosional impact. Original soil shows highest SOC in separates >125μm meanwhile in transported soil highest values occur in smallest fraction <20μm. Respiration rate is largest in SOC in separates >125μm. With regard to the mass balance 41% of soil SOC is eroded in particles >63μm, which shortly will be deposited along the transport path whilst respiration immediately started after erosion and deposition. This fact led the authors to the conclusion, that burial of SOC in deposition areas in global carbon balances is overestimated by around 0.07-0.09 Pg yrˆ-1, which is in the same range as the global sink rate of 0.12 Pg yrˆ-1 confirmed by van Oost et al. 2007. The paper is well written revealing excellent linguistic skills. Selection of references and working with existing state of the art papers is attesting a profound knowledge in SOC dynamics to authors. However, the present paper reveals methodological weaknesses, which prevent to draw conclusions on global scale. To my knowledge, soil aggregation and SOC distribution among particle/aggregate size classes, one soil single sample of a silt loam from northwest Switzerland cannot be sufficient to explain all factors involved. Soil aggregation strongly depends on SOC contents, SOC types, amount and type of Fe-oxides, calcium carbonates and clay content, which underlies high spatial dynamics, depended on soil biota, climatic conditions, soil age and parent material. Based on one soil samples existing global balances cannot be compiled. Rainfall simulation on small plots may result in big aggregates within the transported sediment but regarding erosion on entire slopes those aggregates may break down to smaller sizes due to the impact of surface runoff (expressed as momentum fluxes, shear strength or stream power), which may exceed the impact of falling droplets after few meters by multiples. As a consequence aggregates will break down with accordingly higher SOC contents in smaller sizes, which have larger travel distances. This SOC may be buried in down slope depressions or be exported to channel network. In my mind the applied rainfall simulator is suitable to simulate splash erosion and initial interrill erosion but it is inappropriate to simulate effects of distinct interrill and rill erosion as it appears during natural rainstorms on slope scale. Such processes can only be simulated using runoff reflux approaches using sediment loaded water as it is already published. Beside these limitations the use of one single rainfall intensity is not suitable to cover global climatic conditions with high spatial gradients of possible inter annual rainfall intensities. Short periods of very high intensities then may provoke a faster break down of soil aggregates which lead to the above stated conclusion. In order to describe soil detachment by soil erosion models mechanical dispersed soil samples are strongly require as input parameters. Ultrasound devices, as applied in the present study, are an useful tool to simulate mechanical dispersion as it happens during rainfall-runoff events. However the calibration and comparability of devices is still uncertain. In this regard mechanical dispersed soil texture should reflect the soil texture as it present during erosional transport. The fact that smallest mass (2.3%) of particles/aggregates of original soil is found in class >125μm with highest SOC concentrations (approx. 2.5%) let me assume that most stable aggregates are formed by very stable organic compounds as it appears in earth worm droppings, resisting the impact of mechanical dispersion. In sediment separates of the same class approx. 16% have mean SOC concentration of 1.4%, which is much lower than in the original soil. Accordingly SOC does may not play a significant role for aggregation of these aggregates. In this regard it is uncertain how these aggregates would react during transport over longer distances. If broken down they would be transported over longer distances and deposited in down slope areas or even exported to channel system. Both possible cases contradict the author′s conclusion. If the final conclusion would be correct, why colluvial sites are enriched in SOC? The rapid CO2 emissions from broken aggregates cannot explain this field records. Furthermore the authors conclude, that existing erosion models need an additional erosion parameter for SOC travel distance, disregarding state of the applications using of the EROSION 3D model. Also the LiSEM model can be applied to mechanical dispersed particle classes. Such models usually work with 9 different particle/aggregate classes to cover fine, medium and coarse fractions of clay, silt and sand. In this con-

text the presentation of different classes for original soil and sediment as well as the disregard of clay (<2μm) within the study (table 1 & figure 3) complicates the comparability and limits further use for erosion modelling. Please explain why classification of clay separates is not possible. All in all I would summarize that the present paper is an useful step forward in understanding SOC distribution and transport by erosion processes but based on experimental approach and only one soil sample it is impossible to draw global conclusions. As an experimental paper with profound discussion of methodological improvements and limitations it would be more valuable for scientific community.

Interactive comment on Biogeosciences Discuss., 11, 8829, 2014.