Biogeosciences Discuss., 9, C4275–C4280, 2012
www.biogeosciences-discuss.net/9/C4275/2012/

Biogeosciences
Discussions

Interactive comment on "Density fractions versus size separates: does physical fractionation isolate functional soil compartments?" *by* C. Moni et al.

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Received and published: 25 September 2012

Referee #1

The present paper presents the results of two different physical fractionation techniques (particle size fractionation and aggregate density fractionation) which were applied to a field incubation experiment with N15 labelled litter in forest soil. The authors generate from their data some recommendations with regards to the use of physical fractionation procedures and suggest an improved procedure, which is supposed to isolate organic matter of progressing decomposition from soil in different physical fractions. Although, there might be some valuable information included these are diluted in a conceptual framework, which is in my opinion too complicated and not really necessary. The authors aim to derive functional soil compartment by physical fractionation.

However, physical fractionation procedures will always be biased by their operational nature when choosing the density or particle size limits. Therefore, in my opinion, the authors should more focus on the actual information on N cycling they could derive from the two fractionation procedures they applied.

Authors

We appreciate the thoughtful comments of the reviewer and stand ready to implement such advice as will improve our product, but we feel that the concerns raised in the preceding paragraph are to a certain extent the result of an oversimplification of the admittedly complex issues addressed in the manuscript. For example, the reviewer states:

"the authors aim to derive functional soil compartments by physical fractionation. However, physical fractionation procedures will always be biased by their operational nature". Here the reviewer uses the point that we identify as the major science problem to be resolved by our research"...the fundamental question emerges which soil functions or process regimes are best represented by a given physical fractionation scheme" as an argument to suggest that we were not aware that physical fractionation is inherently operational. We respectfully refute this insinuation. As we state on page 8, our study "...aims at evaluating the specifity and relevance of the information provided by size and density fractionations.....we test the hypothesis that physical fractions may allow the observer to identify functional subunits of the soil fabric and the associated process dynamics of soil organic matter". The quote proves that our objective reaches significantly beyond "deriving functional soil compartments by physical fractionation". The quote further makes it clear (we evaluate the specifity of fractionation based information) that our underlying assumption is NOT that physical fractions might be anything like an absolute category. Rather, we ask whether a given particle size fraction or density fraction can represent a functional reality, and we do indeed find that not all physical fractions live up to the challenge. Another misunderstanding seems to be associated with the role of the 15N label. It is NOT our objective (compare para-

graph 2, page 8) to present a comprehensive study of N –cycling. Rather, the isotopic label is used to identify the extent (quantitatively) to which a given physical fraction is involved in the cycling of organic matter in general and organic N in particular.

Authors response to specific comments

1) English of the whole manuscript should be improved; in particular, the abstract is very badly written and can hardly be understood. (exp the sentence: however, scientists investigating specific aspects of OM are pointed towards ADF when adsorption and aggregation processes are of interest whereas PSDF is the superior tool to research the fate of particulate OM) The aim of the study should be stated in the abstract. After the first sentence, the authors could begin with: The aim of the study was to determine whether physical fractionation. . .. . . In particular we investigated if. . .. . ..

We regret any inconvenience associated with our language skills. However, in response to the reviewers criticism, the senior author (MK) asked several of the native english-speaking faculty at OSU to examine the sentence that was chosen by the reviewer to demonstrate poor language skills on our part. Not all called it elegant, but none of them found that the message was unintelligible. We emphasize our willingness to implement constructive criticism and invite more specific recommendations with regard to language improvements. The simple statement “the english should be improved” is not helpful and can be applied to almost anything that is not written by a language professor.

2) The authors are presenting some kind of literature review on physical fractionation methods in the introduction. This is not necessary, because only two fractionation methods are studied. Thus it is more useful to introduce those two in more detail instead of elaborating some kind of classification of all physical fractionation methods. Therefore the introduction need to be re-written and more focused on the objectives to be addressed in this study.

We respectfully maintain that we are not presenting “some kind of literature review”. We are clarifying what we are talking about, thereby putting the audience in a position to appreciate the physical differences between fractions and the mechanistics of different fractionation procedures. With the associated Figure 1 we present, for the first time, an organized and compact overview of the fractionation literature. One important message that can be derived from our synthesis of the literature is the insight that fractionation procedures should not only be categorized by fraction principle (size versus density) but also by the intensity and number of dispersion steps.

3) The observation that OM of litter becomes increasingly decomposed and associated with the mineral phase is nothing new and was reported many times. The new information that could be added by this study is about decanal timescale and different sites.

We agree and reiterate that the objective of this manuscript is not an examination of the decomposition stage of organic matter, but to determine “which soil functions or process regimes are best represented by a given physical fractionation scheme”

4) The authors try to draw some conclusions on the possibility to derive functional soil compartments among physical fractions (discussion 4.3). However, they cannot conclude on function related to the carbon cycle or total organic matter as they only used a 15N label designed to follow the N cycle. This should in particular be mentioned in the discussion points 4.4, where recommendations are given. During decomposition there might be a decoupling of C and N. By the way: did the authors check that the entire label was incorporated into the mineral soil? The humus form of the experimental sites should be stated – and the organic layer, if there is any, should have been analysed for 15N.

The authors do not try to draw conclusions on possibilities, they actively identify functional compartments. The manuscript does not contain any conclusions regarding the “carbon cycle” or “total organic matter”. The manuscript makes a well constrained

promise in the introductions chapter, and the conclusions drawn are in direct response to the goals and objectives stated therein. The reviewer is referred to the methods chapter for informations about 15N in the litter layer, humus forms etc. This information has been published elsewhere (Zeller et al 1998, Zeller et al 2001, Hatton et al 2012, cited in the manuscript) and is therefore not repeated here.

5) The authors do not discuss the bias, which may be introduced by the use of SPT as a density fractionation agent. SPT as a salt may also be a dispersion agent and may therefore in addition to leading to carbon loss lead to aggregate dispersal.

We are not sure we understand the point the reviewer is trying to make here. As we have pointed out above, all physical fractionation methods are inherently operational, which is the reason why their ability to represent ecosystem functionality needs to be carefully evaluated AND the reason why we conducted this research. The inference drawn in our manuscript refers to fractions obtained after performing the dispersion steps described in the methods chapter. Thus any dispersive effect resulting from the chemical characteristics of the SPT solution is part of the "history" of the fractions obtained. Carbon loss into the suspension as a consequence of dispersion will happen after any form of dispersion. There is no expectation whatsoever that the dispersion efficiency of the PSDF procedure should be in any way similar or comparable to that of the ADF procedure. For this reason there is no experimental "bias" that might result from the use of SPT.

6) P. 21: the conclusion that SOM dynamics cannot be fully understood when using a single step fractionation procedure was known before, as fractionation procedures were developed to address different questions with regards to SOM stabilization mechanisms, e.g. the importance of aggregation for ADF procedures.

We respectfully point out that our statement must be seen in the full context of paragraph 1 of the conclusions chapter. We do not claim in our manuscript that this particular insight should be seen as novel. The statement merely rounds out paragraph

1, conclusions chapter, page 24, which contains a differentiated assessment that is indeed novel ("PSDF is more suited to the investigation of the fragmentation of POM whereas ADF performs better at representing adsorption and aggregation processes").

7) Material and methods: the authors state that the A horizon was collected – however, only 2,5 cm of the A horizon were analyzed – how is this possible after sieving?

Here we acknowledge a mistake, only the first 2.5 cm of the A horizon were sampled and sieved at 2mm and not the whole horizon as it was erroneously indicated in the manuscripts. We will correct this.

8) Results 3.3 and 3.4 should be combined.

We agree with the reviewer and will combine these paragraphs

9) P.15 discussion what is 'step fashion'? P. 18 'absence of contrast of tracer enrichment. . .' ?

We agree that semantic improvement is possible here and will rephrase these terms in a revised version of the manuscript

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Interactive comment on Biogeosciences Discuss., 9, 8405, 2012.