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## ***Interactive comment on “Measuring and modelling continuous quality distributions of soil organic matter” by S. Bruun et al.***

**S. Bruun et al.**

sab@life.ku.dk

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Thanks to Alain Plante for a very qualified review and some excellent suggestions as to how we can improve the manuscript.

We are happy that you find the manuscript provides a good introduction to the limitations of current models and techniques and that you think we lay out a good conceptual framework for how fractionation schemes should be integrated into continuous distribution model. This was actually the purpose of the paper. Hopefully this part has improved further after our revisions in response to the excellent suggestions by yourself and Thomas Wutzler.

With the paper we call for new methodology and suggestions on how to modify or de-

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velop experimental methods that align with the continuous model approach, in other words a paradigm shift not only for modellers, but most pertinently for experimentalists. We believe this warrants a paper in itself. We do not agree with you that “. . . given the large volume of literature recently published on SOM fractionation, it seems reasonable that some previously published would be suitable to use in preliminary tests. . .” Concepts, theory and data collection are intimately linked, so any collection of data will reflect the concepts of the researcher, who has designed the experiment. It is therefore not straight forward to transfer and apply data collections embedded in a "pool model concept" to a "continuous model concept". We have therefore chosen not to present any application of the presented modelling framework to real data - to our best knowledge they do not exist yet - but rather to illustrate its application to constructed data, see Fig. 2, produced in response to the request from Thomas Wutzler. We are currently working on trying out some of the methods that we have suggested and hope to be able to publish the results later. However, as we argue in the introduction, providing experimental evidence that this new approach is fruitful requires the effort of a research community and not a single research group. If you think about the amount of money and time spent on discrete fractionation schemes, this may give you an idea of the endeavour that it takes to find the best way to fractionate continuous distributions. The purpose of the current paper is really just to encourage researchers to think in continuous terms and develop fractionation schemes accordingly – hopefully stimulating the paradigm shift needed for continuous SOM modelling to mature from the abstract to the concrete, as you express it.

We agree with you that the first paragraph is unnecessary and have deleted it and replaced it with a short sentence stating that “Understanding of the dynamics of soil organic matter (SOM) and mechanisms that stabilize organic matter in soil is important for our ability to develop management practices that preserve soil quality and sequester carbon”

In order to state the objectives more clearly we have rephrased the last sentence of

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the introduction to read: “The purpose of the current paper is to discuss the implications of characterizing SOM by continuous quality distributions, present a possible modeling framework that can be used to model the measured continuous quality SOM distributions and review methods that may be employed to measure the distributions.”

We agree with you that laser diffractometry is a useful technique for measuring size distributions, but as you state, it cannot be used to measure the amounts of SOM associated with particles of the different sizes. As you suggest, it may be possible to combine MIR/NIR with laser diffractometry to simultaneously measure particle size and SOM concentration. We have chosen not to explore this possibility in the paper, but now the idea is available in this correspondence.

We take your point that the separation of a continuous density distribution of SOM that we propose is essentially a sequential density fractionation. We have rewritten the paragraph and are now employing the term sequential density fractionation. In addition we have added the highly relevant references you have suggested. We agree that the sequential separation becomes prohibitively expensive when too many fractions are being separated on more than a few soils. As we suggest in the paper, we hope that density gradient centrifugation may make this possible. Simplifications may be possible, but not before a more in depth understanding of the effects of density has been generated which allow us to separate at the right densities.

You are right that adsorption of organic matter on surfaces is considered an important stabilizing mechanism and that surface area therefore is considered to be an important controlling variable by some researchers. However, we do not really see how this knowledge can be used to devise a method to separate a continuum of soil organic matter. Of course particle size is somehow related to surface area as small particles will generally have a much larger surface area per unit of mass and therefore a larger fraction of the organic matter associated with small particles can be sorbed. Therefore we have chosen to mention this more explicitly in the introduction to the size separation which now states: “The sand fraction which is dominated by quartz only exhibits weak

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interactions with SOM. This provides different degrees of protection from microbial degradation. In addition the surface area per mass of soil is much larger for the small clay particles. Therefore the small particles provide a large surface area for sorption of SOM which is considered an important stabilizing mechanism (Guggenberger and Kaiser, 2003; Kleber et al., 2007).”

The section on thermal treatment has been revised, with the very useful input of your recent review on thermal analysis techniques.

We agree with you that the discussion section is somewhat repetitious and unnecessary in a review paper. Therefore we have removed the discussion part from the “Discussion and Conclusions”, and rewritten it into a pure “Conclusions” section instead.

We agree that changes in Fig. 1 during a time step are small and although the changes taking place in a time step are indeed small they may be too subtle for illustrative purposes. Therefore we have enhanced them in the figure.

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