

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

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We have known for some time now that kinetically-based compartmental models of SOM dynamics are both incomplete in their conceptual description of the range of composition of SOM, and difficult to parameterize. Continuous quality distribution models have thus always been highly attractive alternatives, but also suffer from difficulties in parameterization. It was with a degree of anticipation that I undertook the task of reviewing the Bruun et al. manuscript “Measuring and modeling continuous quality distributions of soil organic matter”. While the body of knowledge based on a multitude of SOM fractionation schemes has grown significantly, the development and adoption of continuous quality models has lagged. The concept of marrying fractionation to the quality continuum of SOM is of significant scientific interest because of implications

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in improving our ability to predict SOM responses to disturbances such as climate change. However, the manuscript by Bruun et al. suffers a major deficiency – a lack of data.

The manuscript does well to introduce the limitations of current models and techniques, provides a good review of the continuous distribution model, and lays out the conceptual framework for how fractionation schemes might be integrated into continuous distribution models. However, I find that the manuscript represents a review of current knowledge and limitations rather than a significant advance. Much of the language used in the manuscript describes a potential reality. For instance the abstract contains the following phrases: “should be developed”, “should be incorporated”, “should hold value”, “will have to be developed” and “will be a major task”. It would seem to me that those best qualified to undertake these challenges are these authors, and yet the manuscript presents no real data to test any of the ideas presented. 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.

Overall, I find that this manuscript has not generated significant new insights because of the lack of concrete tests using real data. Perhaps the manuscript is premature and should be allowed to incubate while either experiments are performed or preliminary tests can be performed using published data. The continuous distribution SOM model has remained in the abstract for too long, and I would very much like to see it mature. While there may some intrinsic value in such a “thought piece”, I strongly believe that the contribution of this manuscript to our understanding of SOM dynamics would be greatly enhanced with experimental/modeling data.

Specific comments

First paragraph of the introduction: Practically every manuscript that concerns the study of SOM dynamics begins with a paragraph similar to this. Is it really necessary at this point? I would recommend omitting this paragraph and beginning the manuscript

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at Line 12.

The introduction does not contain a statement of the objectives for the study. The abstract ends with a sentence that could be construed as an objective, but I find “spawning interest” to be unsatisfactory. I would recommend that more specific objective be outlined near the end of the introduction.

5.1.1 Particle size fractionation: The authors might consider a discussion of laser diffractometry as a potential method to rapidly generate continuous functions of soil particle size. However, while diffractometry can generate distributions of particle size, it cannot provide the amounts of SOM associated with those sizes. And thus we are left with the tedious task of using a large number of sieving steps. If the authors choose to remain on the speculative side, they might propose the development of an instrument that would combine MIR/NIR with laser diffractometry to simultaneously measure particle size and SOM concentration.

5.1.2 Density: The authors propose what is essentially sequential density fractionation. A number of studies have been recently published using this technique that are not cited (e.g., Baisden et al. (2002, GBC); Sollins et al. (2006, SBB); Sollins et al. (2009, BGC)). There is, however, a significant practical barrier to the development of a near-continuous sequential density fractionation scheme – cost. The amount of SPT and labor inputs required per sample for even a simple sequential density fractionation is significant enough that a number of researchers have decided that a simpler scheme is more practical.

5.1.5 Surface charge: In addition to surface charge, recent research suggests that specific surface area is an important controlling variable in the stabilization of SOM. The authors might consider how this might be incorporated into a continuous model.

6. Discussion and conclusions: Much of the text in this section is a repetition of what has been reported in previous sections. The discussion would be greatly strengthened by reporting preliminary tests using actual data.

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The last paragraph begins with “In conclusion, there seems to be a range of potential...”. I don’t find this to be particularly satisfying. It is somewhat vague and inconclusive. I would recommend strengthening the conclusion with more concrete statements. I would agree with the authors’ conclusion that “fractionation according to size and density and thermal treatment (are) the most promising”. However, the labor and cost inputs of developing near-continuous functions of SOM quality using size and density currently make this approach impractical. Thermal analyses have shown great promise in being able to describe the complete quality continuum of SOM, but there remain significant challenges in the interpretation and quantification of thermal data (see Plante et al. (2009, Geoderma)).

Figure 1: As these are conceptual figures, would they not be of greater value if the differences in the SOM quality distributions were less subtle?

Technical corrections

Line 15 of the abstract: replace “leads” with “lead”.

Line 11, Page 9048: I believe the word “sod” should be replaced with “soot”.

Line 13, Page 9057: Replace “provide” with “provides”.

Line 12-14, Page 9070: Replace “is the most” with “are the most”, and “offers new” with “offer new”.

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