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Interactive comment on "DRIFTS peaks as measured pool size proxy to reduce parameter uncertainty of soil organic matter models" *by* Moritz Laub et al.

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General comments

The papers deals with initialization of pools in the soil organic matter model of Daisy. The paper is using unique datasets for long-term fallow treatments to test a new way of initializing the soil organic matter pools based on specific peaks in the DRIFTS specta of the soils. Pool initialization of SOM model is an important issue that is still causing some difficulties with the currently used approaches. The paper is therefore very timely and present an interesting approach that could be useful in many situations. The work is of a high quality and based on high quality data and the manuscripts is well written.

Specific comments

Line 78. I agree that the DSI can be better than the steady-state assumption, but perhaps it is worth discussing this in a little more detail. If information about the history of the site is available then that method should work. This require that the history is known for millennia, and that is rarely the case.

Line 118: Was soil samples from through out the experimental period analyzed? Please specify.

Line 129: The spectra were not recorded in absorbance, but subsequently converted to absorbance units, right?

Line 130: I wonder how much this way of determining the DSI is affected by the instrument i.e. if somebody took the same soils and did the measurement on another instrument would the get the same DSI and pool sizes. I am afraid that it would be quite much affected by that especially if you use other IR detection techniques. Maybe it would be worth addressing this in the discussion.

Line 181: It says 84% and not 83% in Table 2. Please correct where appropriate.

Line 196 to 209: I am not entirely sure I understand what the point of analyzing the SMEx with a statistical model is. I think you should consider whether it add enough understanding to warrant inclusion. Alternatively explain the point a little better.

Line 236-237. The necessity of constraints on the fSOM-Slow parameter is a little problematic. I cannot help thinking that it means that the data, which is used for calibration, is insufficient. With these restraints, I guess you are likely to end up with a value of 0.35 which is rather arbitrarily chosen by you.

Line 364-365. I agree that even though we have had the same management for a long time the steady-state assumption is not valid. However, I believe that the reason for this has to do with longer-term effects rather than the smaller effects that you mention i.e. variation in climate agricultural management. If you look at a longer terms, most sites would probably have been deforested within the last 2000 years. Because of the high inputs from the forest, this could have resulted in an unusually large fraction of resistant organic matter that has not been degraded from that period. Also it is very common with drained soils soil. This means that the soil at some time it its history has had a very high water table and perhaps even been inundated. We know that this can result in significant accumulation of organic matter. After the soil has been drained, this has led to a large residual of resistant C again. The same could happen if there has been a history of fires with inputs of charcoal. Perhaps this is worth discussing a bit more.

Line 373: I cannot help it thinking that it is somewhat of a coincidence that you get better model performance with the DSI as long as you have not recalibrated the model. Of course using more data as for example DSI to restrain the model should improve the model, but only after it has been recalibrated.

It is not entirely clear what data were used for the calibrations based on DSI. As far as I understand, you measured DSI of all the soil samples and that means that you can compare the simulated distribution between fast_SOM1 and slow_SOM with the one measured and calculated using formula (2) and a similar formula for fast_SOM. Is this the case? And if it is why have you not shown the “measured” value of fast and slow SOM and compared it with the modelled?

Is it worth publishing the optimal parameters selected by the Baysian calibration based on DSI?

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