Biogeosciences

Discussions

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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.*

Anonymous Referee #2

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General comments

Laub and Colleagues present interesting ideas how DRIFTS spectra could be used to initialize and calibrate soil organic matter models. What warrants more discussion is that with their results we should put again more emphasis on the chemical recalcitrance hypothesis, i.e. that molecular properties determine the persistence of organic matter in soils. The literature seems to disagree (Schmidt et al., 2011). If we indeed assign the aromatic peak to slow cycling pools with a turnover time of 426 years and the aliphatic peak to a fast cycling pool with 47 to 90 years, the authors would contradict the synthesis of Schmidt et al. (2011) (their Figure 1, for example). In my opinion, it

would be interesting if the authors could at least discuss how their DRIFTS peaks could be useful for the new class of microbial-mineral models such as Tang and Riley (2015) or Sulman et al., (2014).

Specific comments

The authors state that "the DRIFTS initialization of SOM pools significantly reduced model errors of poor performing model runs assuming steady state, irrespective of the turnover rates used, but the faster turnover parameter set fit better to all sites except Bad Lauchstädt. This suggests that soils under long-term agricultural use were not necessarily at steady state." In my opinion this statement is not backed up by their results. The Bruun parameters with steady state assumption perform better at Ultuna and Kraichgau + Swabian Jura (Table 4) for SOC stocks.

The authors also state that "[. . .] two approaches [. . .] significantly reduced parameter uncertainty and equifinality". One of the approaches was the inclusion of DRIFTS. But looking at the violin plots in Figure 5, only the humification efficiency seems to be better constrained. I suggest modifying the statement towards this direction.

I agree with the other reviewer, Sander Bruun, that analyzing the squared model errors with a statistical model should at least be better explained.

The manuscript would benefit from a thorough spell and language check.

Schmidt MWI, Torn MS, Abiven S et al. (2011) Persistence of soil organic matter as an ecosystem property. Nature, 478, 49-56.

Sulman BN, Phillips RP, Oishi AC, Shevliakova E, Pacala SW (2014) Microbe-driven turnover offsets mineral-mediated storage of soil carbon under elevated CO2. Nature Clim. Change, 4, 1099-1102.

Tang J, Riley WJ (2015) Weaker soil carbon-climate feedbacks resulting from microbial and abiotic interactions. Nature Climate Change, 5, 56-60.

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