

Interactive comment on “Tillage-induced short-term soil organic matter turnover and respiration” by S. R. Fiedler et al.

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Soil-2015-91 presents a ‘brave’ (because challenging) attempt to finally better understand what exactly happens with specific OM constituents after soil tillage. The presented work is certainly complementary to a multitude of studies that have merely measured respiration or shifts in soil physical properties and environmental drivers. The similarity of mass spectra and thermograms of three different field objects really shows that the applied Py-FIMS technique’s output is robust. And this renders it fit for detection of expectable small shifts in SOM composition in short term studies. The paper is well written, but I was not always convinced with explanations provided for a remarkable interactive effect of tillage and digestate application on soil CO₂ efflux. So in part it seems that some elements of the discussion need to be altered. But in general, the present paper is in good shape and only requires minor corrections. The

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re-confirmation that sterols may inhibit microbial activity in soils really warrants further investigation. One suggestion for the data-analysis: to maximally explore linkages between (shifts in) the m/z datasets by tillage and (shifts in) CO₂ efflux, advantage could have been taken from newer statistical techniques like partial least squares regression. This would have resulted in principal components that maximally explain variation in CO₂ efflux (the dependent) and not in a set of components aimed at describing variation in the m/z data.

L 19-20 'Before tillage, BD showed much more volatilised matter (VM) during pyrolysis, indicating an increased amount of SOM.' Not really mention worthy in an abstract

42 suggest to replace by 'readily biodegradable OM' instead of 'labile organic matter', an ambiguous concept

52 'which SOC constituents that form the majority of SOM are mineralized' reads strangely, please rephrase

56 better: 'long-term stability in soil is still under debate. . .'

2.1 please provide sand, silt & clay percentages

115 so is this then a mouldboard plough?

143 all between the () appear to be too much detail

161 'see 2.3', is written in 2.3, so where is this reference pointing at?

The purpose of measuring HWC and HWN should be better motivated in the introduction/M&M, when readers are mainly convinced that the whole intent of the present study was to understand which OM building blocks are preferentially degraded by consequences of tillage soil disturbance.

178 The reference is here part of your sentence and should not be expressed as (Leinweber et al., 2013) Leinweber et al. (2013) Correct these mistakes in annotation throughout the tekst, several other examples follow later on in your text, e.g. 57 Möller

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et al. (2015), 309, 320. . .)

Table 1 Designation of significant differences by the superscript letters is not clear. Is this the outcome of an ANOVA on all 9 treatment/date combinations? Seems that way, but be clearer in the caption text

Fig 3 Not clear what time intervals these cumulative CO₂ emissions represent. Should be indicated.

254 Indicate here in the text also the direction of 'differences' in the abundance of m/z 55, 60, etc.

282 presumably the authors are mentioning increases in the TII proportion of major peaks for carbohydrates?

Fig 6 Why were correlations with the CL treatment's CO₂ efflux, HWC and HWN not displayed?

308 'BD'

350-353 A strange explanation: in the BD plots you also expect labile 'physically protected C' not contained within digestate to be equally present as in the MF plots, next to of course labile C in the amended digestate. So lifting of bio-availability of previously 'entrapped' labile OM should equally have occurred in the BD and MF treatments, equally contributing to a short-term CO₂ efflux. So I propose to reformulate this section. The microbial use efficiency theory which follows looks much more plausible. But then again, the HWC/HWN ratio was in fact much lower in all MF samples, and yet CO₂ efflux was higher. So in the end I think part 4.2 should end with an acknowledgement that the described mechanisms do not well explain why CO₂ efflux was lower after tillage in the BD when compared to the MF plots.

410 build-up

411 The link between trends of carbohydrates and heterocyclic N is not clear. This

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statement requires further explanation.

401-409 Reads like a plausible explanation for the observed shifts in SOM biochemical composition. Can trends of individual m/z indicative for carbohydrates or peptides confirm that the short-term build-up of these substances is indeed through production of microbial biomass and metabolites?

771 suggest ‘...and time of tillage operations...’

780 indicate significance level

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