

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

Anonymous Referee #2

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Tillage-Induced short-term soil organic matter turnover and respiration

The MS addresses tillage-induced short-term CO₂ emissions and alterations of SOM under various fertilizer regimes by applying pyrolysis-field ionization mass spectrometry. The main problem with the MS is the lack of statistical replication of the fertilizer treatments; yet the authors refer extensively to statistical inferences, but based on tests with pseudo-replicates, which is generally considered as invalid. While the statistical design hampers valid comparisons between fertilizer treatments, it could be argued that effects of tillage operations within each fertilizer treatment could still be analysed, and the MS should be reworked with emphasis on this. At least, depending on journal standards, the authors should discuss the implication for the conclusions that comes

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from using inferential statistics on pseudo-replicated data. The discussion at times is too speculative in relation to interaction between organic C (and N) pools and microorganisms (e.g., 423-437). Fewer points, with adequate support from the experimental data, should be focused on.

37: Rather: ‘...by the disruption of macroaggregates, leading to release of protected SOM...’

44: Remove author initials in references

52: The way the link between SOM and SOC is expressed sounds odd also in the reply to R1 (‘Admittedly, SOC accounts for the majority of SOM, but...’). And maybe there is no need to introduce this issue here (but see Pribyl, 2010). To simplify, I would rather suggest to write: ‘However, these correlations do not causally explain which organic components are mineralized.’

88: Although with some updates, this section (88-100) is closely following Fielder et al. (2015), and some condensation would be appropriate, e.g., replacing 91-95 by: “The top soil (0-30 cm) had an organic C content of $1.16 \pm 0.1\%$, pH of 7.4 ± 0.9 , and bulk density of 1.51 ± 0.08 (mean \pm standard deviation, $n = 3$) as measured according to Fiedler et al. (2015).” Also, for such an important parameter as pH for mineralisation, it would be reassuring (and easy) to provide more robust information than pH 7.4 ± 0.9 . As a detail, always give same no. of decimals in mean and measure of variability (i.e., adjust $1.16 \pm 0.1\%$).

108: As the BD was applied to 160 kg N/ha in 2012, it would seem the assumption of 70% available N in total N was founded before the personal communication in 2014. The assumption is ok, but the writing could be made more consistent.

112: What is meant by ‘in original matter’?

112: Suggest completing the chronological description: ‘During the cropping season 2012, maize was grown according to conventional agricultural practice.’ (and add if any

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special management were implemented)

118: Give dimension (b x w) for collars

120: Maybe better: 'The adjacent collars were placed 1 m apart.'

128: Yes, even though acknowledged, pseudo-replication is of course a weakness of the study. And the HSD statistics as shown, e.g., in Table 1 are severely compromised by this design and should be reconsidered and preferably omitted

141: Suggest splitting sentence to improve readability: '... of a concentration measurement. This allowed discarding data...'

159: Does this literally mean 5-15 cm depth or is it 0-5 to 0-15 cm? Please specify. In the former case, why was the top-soil not considered?

218: For N only partly true and for HWN claim not statistically supported by data (even with faulty HSD)

166: Since 5 mg samples were used, a full description of procedures for homogenisation and subsampling should be given

191: What is meant by 'total nitrogen bound' ?

273: '...but it was not significant (p < 0.01)'. Correct p value ?

298: rather '...in contrast to...'

299: '...to phenols, lignin...' or rather '...to phenols and lignin...' i.e., as individual compounds or as a group?

301: Correlation to free fatty acids not shown in Fig. 6 (free fatty acids not included in figure)

306: Don't abbreviate Table

306: See comment to 218

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322: Speculative argument (322-323)

344: No need to invoke such ratios; suggest to delete 344-346.

348: Less available than what? You don't have a reference with the native biomass. Indeed this section is too speculative and should be shortened (347-365).

370: Leinweber et al. (2009) is not included in reference list

370: 'complementarily' – do you mean, e.g., 'collectively'

385: be specific: 'The VM increase...'

442: 'Since microbial activity can affects heterotrophic respiration...' this seems to be an understatement; delete 'can'

449: Again, this add to the many speculations put forward in the discussion (449-451)

455: I don't think the data has shown that lower CO₂ efflux was related to higher C use efficiency?

463: An awkward formulation: 'which likely inhibited an enhanced microbial activity.' Maybe use 'prevented' rather than 'inhibited'. Anyway the C/N ratios shown in Table 1 seem not to be critical for microbial mineralisation; rather N mobilisation would occur from mineralization.

Reference list is somewhat excessive (74 refs) with 44 unique references in the discussion alone, underlining the need for a more stringent focus in the discussion.

666: Ohkubu et al. (2012) not cited in text

560: add editor info

567: for consistency spell out journal names (also 590, 647, 565, 709, 736)

Table 1: Define in Table caption the meaning of pre, post and post+4

Table 2: As for Table 1

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Figure 1: Spell out rev. plough in caption

Figure 2: '... (harrowing to ~10 cm depth and ploughing to ~30 cm depth)' '...in order to better visualize the tillage effect' Indicate specifically what boxes, error bars, points and symbols refer to in this case

Figure 6: This design is not helpful at all to the reader; please indicate R values numerically rather than by area-based symbols. And add explanation of acronyms.

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