

Interactive comment on “Short-term effects of fertilization on dissolved organic matter (DOM) in soil leachate” by Alexandra Tiefenbacher et al.

Anonymous Referee #1

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This manuscript reports a study on the effects of calcium ammonium nitrate and pig slurry on the composition and concentration of dissolved organic matter in two soils. In general, the manuscript is well structured and written. However, there are many studies in the literature covering this topic and using similar approaches (e.g., Tye and Lapworth, 2016; Li et al., 2019; Seifert et al., 2016), some cited by the authors. Other major concern is that only two soils were used. This hardly represents a wide variety of soil textures and makes the results hardly generalizable.

Technical and typographical comments and suggestions

Title. Remove “(DOM).” No need to define or use abbreviations in the title.

L. 27. “highly dependent on”

C1

L. 28. This last sentence is not clear. Please clarify.

L. 38. “generously dimensioned fertilizer application rates” Please reword.

L. 46. Please clarify what you mean by “light availability.”

L. 58-59. Why are these results contradictory?

L. 73. “. . . water flow.” Provide a reference.

L. 87-89. If the novelty of the study relies on the use on undisturbed soil cores, the results should have been compared to those obtained using disturbed cores.

L. 90. The reasons for these hypotheses need to be better explained (e.g., why do you expect higher DOC concentrations in coarser soils?).

L. 114. Clarify how the different sampling times could have affected the results.

L. 133. Why this specific temperature and this specific humidity?

L. 133-134. The authors convey in the introduction that they intend to make the study as realistic as possible (L. 87). So why keeping the soils in the dark?

L. 156. Soil organic C analysis needs to be conducted on finely ground samples (not on 2-mm-sieved soils).

L. 182-195. Were inner filter effects considered for calculations based on fluorescence?

L. 200. Check the R version used (the version reported seems too old for 2019).

L. 225 I suggest reporting the exact p-values.

L. 231. Report in mg per g of soil, or similar.

L. 317. This is surprising indeed. The generalizability of these findings and their implications needs to be better discussed.

L. 373. “higher microbial activity.” This is not composition as revealed by fluorescence.

C2

Please reword.

L. 377-378. I don't follow this explanation. Microbial decomposition should degrade first the most labile compounds, thus leaving behind highly recalcitrant organic matter, with high molecular weight and aromaticity.

L. 392. What do you mean by "harmonizing DOM composition"?

L. 398. Remove "even."

L. 415. But your results suggest just the opposite, that agriculturally derived DOM will decrease in aquatic ecosystems.

L. 424-425. Again your results suggest the opposite.

It would be beneficial to show some representative fluorescence spectra.

Interactive comment on SOIL Discuss., <https://doi.org/10.5194/soil-2019-97>, 2020.