

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

Anonymous Referee #2

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In this manuscript, the results of a leaching experiment using undisturbed soil columns were presented. The authors aimed to reveal the effects of mineral and organic fertilization on amounts and composition of DOM in dependence on soil texture. There are many published results available in the literature showing quite contrasting results related to this topic. I do not see that the results of the present study increase our knowledge about effects of fertilization or soil texture on DOM leaching. The use of only two different soils does not allow general conclusions.

In addition to the limited contribution of the study to increase our knowledge about dynamics of DOM in agricultural soils and their controls, the experimental approach is not convincing. First, the authors compared effects of fertilization in dependence on soil texture using two soils sampled at different seasons. One soil was samples in

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early spring whereas the other soil was sampled in fall. Taking into account the strong seasonality of DOM dynamics in soils (amounts and composition) it is very difficult to compare the results of the two soils. Second, the authors did not give any information about the used pig slurry, e.g. DOC concentration, DOM composition. Therefore, it is impossible to assess effects of organic fertilization if amounts and composition of added organic matter is not known. In addition, the studied two soils differ in pH (1 unit). A higher pH should be related to a higher solubility and could superimpose assumed effects of fertilization and soil texture.

I have some further comments related to the methodological approach and the text of the manuscript: Line 25: The authors did not determine molecular size – the methods they used might indicate some changes occurring during the experiment. Line 64: The term stabilization is used in two different aspects; (i) solubility / desorption and (ii) decreasing microbial decay Line 93: Justification of the hypothesis is needed. Line 109: The combination of a loamy sand, 0.8% of organic C and a Haplic Chernozem is not a very common one. Line 185: The authors did not describe any correction of the fluorescence spectra, e.g. for inner filter effects. In their figures, the authors mixed up soil type and soil texture.

Although the topic of this study might be of high interest for the readership of SOIL, I cannot recommend publishing because of the indicated weaknesses and the limited new knowledge they provided. The authors might think about the main story they want to tell. Then they might consider re-writing the manuscript as a short communication taking the limited character of their study into account.

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