

Interactive comment on “Organic carbon content in arable soil – aeration matters” by Tino Colombi et al.

Anonymous Referee #1

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General comments The authors present an empirical analysis of the relation between soil organic carbon contents measured under no-till, conventional and organic farming practice and static soil physical properties. The manuscript is well-structured and written in a concise style. However, there are major methodological concerns. There is no evidence that the differences in SOC are related to the farming practice. 10 sites were chosen for each farming practice. Since SOC measurements prior to the change in farming practice are not available it is not clear whether the SOC changed as a result of the farming practice or whether this is just a result of a random selection of 30 sites. This is corroborated by $p < 0.1$ for a significant difference in SOC according to management (table 2). The topic given in the title and the hypothesis stated in the introduction could not be tested with the data set presented in this study since most

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relevant factors are not included. The SOC content is in equilibrium between the C inputs (not measured) and the decomposition of C. The decomposition process strongly depends on soil temperature. The second most relevant variable is water stress (often expressed by effective saturation or water-filled pore space WFP, e.g. Manzoni et al., 2012, Ecology 93). Even if the same temperature regime is assumed, the soil water status probably differs considerably between the sites and treatments investigated in this study. Both factors, carbon inputs and soil water status are not included in the data-set presented in this study. The third most relevant factor might be soil aeration. Of course, when the most relevant factors were excluded from the statistical analyses, soil aeration appears to matter... And soil water content at sampling or water holding capacity are surely not sufficient to account for the effect of water stress on microbial SOC decomposition. Soil aeration deficits in agricultural topsoils are very unlikely. Strong effects of oxygen deficits were observed at O₂ concentrations < 0.04 cm³/cm³, e.g. Głinski Stepniewski, 1985, Soil aeration and its role for plants. This indicates that the topsoil has to be saturated almost entirely with water, what rarely happens. This is related to very high precipitation events only, and even then in a structured topsoil with macropores oxygen supply could be sufficient due to the diffusion of oxygen into water-saturated aggregates (Højberg et al., 1994, Soil Sci. Soc. Am. J. Diffusion coefficients in near-saturated, structured soils are high, see Kristensen et al., 2010, J. Contam. Hydrol. 115. Soil aeration is a complex and temporally and spatially highly variable process. In order to test the hypothesis stated in the introduction a data set comprising carbon contents at the beginning and the end of the experiment, time-series of soil temperature, time-series of soil water contents, time-series of soil CO₂ concentrations, time-series of O₂ concentrations or at least redox potentials and time-series of soil heterotrophic respiration are required. I suggest to re-analyse the present dataset with a new focus.

Specific comments p2 25 there are approaches that account for macropore tortuosity and their effects on gas diffusion, e.g. Kristensen et al., 2010, J. Contam. Hydrol. p2 29-31 yes, but this means that increased aeration will lead to lower SOC, since decom-

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position rates would be higher. This is a clear contradiction to what is hypothesized in the abstract: '...that improved soil aeration, which is strongly controlled by soil structure, leads to higher soil organic carbon content.' p3 13-15 I strongly disagree. There is a bunch of literature (actually an entire community) that found soil temperature and secondly soil water content to be the most relevant drivers of carbon turnover in soils. p3 23 but how will you separate the confounding effects of increased aeration and limited water ability for decomposition? Both are highly inter-related. Low water contents, leading to decreased SOC decomposition, are inherently linked to increased soil aeration and vice versa. p3 16-28 a clear mechanistic description of the processes and the status variables that affect soil aeration and its consequences on SOC decomposition is missing p4 1-2 do you really expect measurable and significant differences in SOC after 5 years? There is a clear lack of data on C inputs. p6 1-2 water holding capacity is not a good proxy for the dynamics of soil water content or water-filled pore space p8 13 exactly, there is considerable overlap, which also causes a rather higher error probability ($p < 0.1$) ... p8 24-15 What would be the effect of increased porosity/water holding capacity? The same amount of water (precipitation) infiltrating into a larger volume will cause less water-filled porosity. This in turn will cause less SOC decomposition, which subsequently leads to higher SOC contents. I suspect a spurious relationship between air permeability/gas diffusivity and SOC. I assume the true correlation is between water-filled porosity and SOC content. p10 9-10 I strongly disagree. The effect of soil aeration on SOC decomposition is well documented in literature. This is text book knowledge, see Glinski Stepniewski, 1985, Soil aeration and its role for plants, chapter I, section II, A.4 and A.5 p11 5-6 This is highly deculative. Neither organic matter inputs nor the main drivers of decomposition were included in the data-set. p11 28-30 This statement is probably one of the main conclusions of this study. However, neither carbon inputs nor the stimulation of carbon decomposition was measured in this study. This conclusion is speculative at this point and not related to results presented in this study.

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