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

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Reviewer comment: General comments The authors present an empirical anaylsis 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 releated to the farming practice. 10 sites were chosen for each farming practice. Since SOC measurements prior to the change in farming practice are not availble 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).

Response: From this very first comment we draw the conclusion that our main findings were not communicated clear enough in the original submission. We therefore made numerous changes throughout the manuscript (cf. responses to the other comments) in order to better emphasize the aims and major findings/conclusions of our study. In very general terms: The primary aim of the study was not to evaluate whether farming practice affects soil organic carbon content and/or sequestration but to "investigate relationships between gas transport capability and organic carbon content in arable soil covering a range of textural compositions" (cf. last paragraph of the Introduction pg3, L21-27). We chose this focus since soil aeration/gas transport properties is often not included into assessments of soil quality (cf. Figure 2 and Figure 4 in Bühnemann et al., 2018, SBB), even though it is known that gas transport properties of soil control soil-atmosphere gas exchange and hence oxygen concentration in soil air, which greatly matter for all kinds of biological activity. We address this issue more specifically in the comments below. Further on in the manuscript we present the effects of the different soil management systems on soil organic carbon content as well as soil physical properties including gas transport properties, of which certain were significant (LSD-test at $p < 0.05$ and < 0.01, Figure 2 and 3, Table 3, Supplemental Figure S1 and S2). However, we now clearly state at multiple occasions that there was considerable overlap between the management systems (pg1 L23-24, pg8 L12-13, L21-23, pg8 L33-pg9 L1, pg10 L21-22) as well as large variation between individual fields for most soil properties assessed (pg7 L18, pg8 L1-2, L23, L32, pg10 L18-21, pg11 L1-2, summarized in Table 1 and Table 2). As the presented study was carried out on-farm, the management systems are likely not as contrasting as in field plot experiments, where typically one or two factors are altered while the rest of the management is the same for the different treatments. Furthermore, variability in soil texture among sites (cf. Table 1), which occurred here but usually does not occur in field plot experiments, showed a significant effect on a range of soil properties (Table 3, Figure 2 and 3, Supplemental Figure S1). Therefore, the effects of soil management on different soil properties (e.g. soil organic carbon content, gas transport capability, etc.) will not be as pronounced as

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in field plot experiments. Moreover, we did not aim to make statements about carbon sequestration, i.e. comparing organic carbon content before and after conversion from one to another soil management system. In fact, all the investigated farms established their current management more than five years ago (this is why we also state that “The fields were managed following three different management systems: conventional (integrated) farming with no-till practice since at least five years, conventional (integrated) farming with tillage since five or more years, and organic farming with tillage since at least five years”, pg3 L30-pg4 L2).

Reviewer comment: 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 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 experessed 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 relveant factors were excluded from the statistical analyses, soil aeration appears to matter...

Response: To address this, we rephrased the original hypothesis in a more conservative way that –we think– takes into account the uncertainties associated with our study (pg3, L21-22). We emphasize at multiple occasions the “equilibrium” between carbon inputs and decomposition (pg2, L3-5, pg3 L6-15, pg11 L29-34, pg12 L1-5, L12-13, L25-30, Figure 5). Hence, we acknowledge the important role of both carbon inputs (from amendments, crop residues, aboveground litter and roots) as well as decomposition for soil carbon dynamics. Even more importantly, with the current paper we did not aim to state that soil aeration (and hence gas transport properties) is more important

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than other phenomena for soil organic carbon content, but to show that it matters. We hope that our adjustments in the text (pg 1 L19-20, pg2 L5-L7, pg11 L29-31) could clarify this.

Reviewer comment: 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 concetrations < 0.04 cm3/cm3, e.g Glinski 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 watersaturated aggregates (Hojberg 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-sereis of O2 concentrations or at least redox potentials and time-series of soil heterotrophic respiration are required.

Response: Yes, soil aeration is a highly complex process and is difficult to quantify. This is also why we did not use proxy values (i.e. air-filled porosity, water-filled porosity, degree of saturation) to assess soil aeration but measurements of gas transport properties (i.e. gas diffusivity and air permeability). In doing so, we account for the effects of pore connectivity and pore tortuosity on gas exchange dynamics in soil, which cannot be accounted for when using measurements of air-filled (or water-filled) porosity (pg2 L18-24, and pg9 L17-21). Regarding the comment about limiting O2 concentrations for root growth: Here, we disagree with the comments provided by the reviewer #1. There are numerous studies from the plant science community (ranging from ecophys-

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iology to agronomy and plant biochemistry), which show that root metabolism undergoes drastic changes and root growth rates decrease at relatively moderated levels of soil hypoxia (O2 > 10%) and/or very short periods of anoxia (flooding for a couple of hours). Other studies showed that this happens on a regular basis in arable soils. We emphasized on that in the text and provided a number of references (pg. 3 L9-12, pg11 L12-15).

Reviewer comment: I suggest to re-analyse the present dataset with a new focus.

Response: Following this comment and the comments raised by reviewer #2, we made numerous changes in the analysis of the data. We now included data on exogenous carbon inputs, both from crop residues and organic amendments, of the last five years (methodology is explained in pg5 L22-31 and pg7 L5-7). In doing so we could show that the amount of exogenous carbon input was similar across the three management systems (pg9 L5-6, Figure 3). Even more importantly, we observed no significant relationship between these inputs and soil organic carbon content (Supplemental table S9-S13), which is highlighted in the Results (pg10 L1-3), Discussion (pg11 L22-24) and Conclusion (pg12 L22-23) sections. Based on these results we suggest that differences in root growth significantly contributed to differences in soil organic carbon contents among fields (not between management systems), (pg10 L7-8 and pg11 L10-20, L22-26).

Specific comments

Reviewer comment: p2 25 there are approaches that account for macropore tortuosity and thir effects on gas diffusion, e.g. Kristensen et al., 2010, J. Contam. Hydrol.

Response: We agree with that and emphasize on the need to measure gas transport properties directly in the Introduction (pg2 L18-24) and the Results (and pg9 L17-21) in order to account for pore tortuosity and connectivity.

Reviewer comment: p2 29-31 yes, but this means that increaed aeration will lead to

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lower SOC, since decomposition 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.'

Response: As emphasized in an earlier comment by reviewer #1 soil organic carbon content results from the balance between input and decomposition of soil organic matter. With the statement given here (now on pg3 L13-15) we try to make it clear that increased soil aeration might also fuel microbial activity and therefore lead to decomposition of soil organic carbon.

Reviewer comment: 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 relvant drivers of carbon turnover in soils.

Response: Please refer to our response above, in which we try to make clear that we did not aim to state that aeration is the only, or the most important property regulating carbon cycling in soil, but to show that it matters (pg 1 L19, pg2 L5-L7, pg11 L28-31). However, plants represent the only possible source for soil organic carbon besides amendments. Plants are highly responsive to changes in soil oxygen (pg1 L19-20, pg3 L9-13, pg11 L12-15), hence soil aeration is of importance. The same applies of course also for the effects of oxygen concentrations in soil air on microbial growth and activity and thus the decomposition of soil organic carbon (pg1 L19-20, pg3 L13-15, pg11 L28-35, pg12 L25-26). Our empirical data together with existing literature provide evidence that soil aeration is an important factor (but not the only one) for soil carbon cycling.

Reviewer comment: p3 23 but how will you separate the confounding effects of increased aeration and limited water ability for decomposition? Both are highly interrelated. Low water contents, leading to decreased SOC decomposition, are inherently linked to increased soil aeration and vice versa.

Response: We hope that our rephrased last paragraph of the Introduction addresses this comment (pg3 L21-27). As stated above, we did not aim to play soil aeration as a

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driver for soil organic carbon content off against other drivers, but to provide empirical and conceptual evidence that soil gas transport properties have an effect.

Reviewer comment: 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

Response: We see this comment related to one of the comments provided by reviewer #2. Therefore, we adjusted the Introduction in order to make it clearer how soil aeration, biological (both auto- and heterotrophic) activity and soil organic carbon content (both inputs and outputs) are related (pg3 L6-15).

Reviewer comment: 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.

Response: No we do not and this was not the aim of the study. Please see our responses to the general comments. We completely agree on the need for data on exogenous carbon inputs (was also raised by reviewer #2) and we included this into the manuscripts as stated above.

Reviewer comment: p6 1-2 water holding capacity is not a good proxy for he dynamics of soil water content or water-filled pore space

Response: We completely agree with this but we did not use it for that. We used it as an additional soil physical property that has implications for root growth and is associated with soil structure (pg2 L15-16, pg8 L26-28, pg11 L26-28)

Reviewer comment: p8 13 exacly, there is considerable overlap, which also causes a rather higher error probability ($p<0.1$) ...

Response: Please refer to the responses to the general comments and our adjustments made that emphasize on the overlap between management systems (pg1 L23, pg8 L12-13, L21-23, pg8 L33-pg9 L1, pg10 L21-22). As stated above, we present results from an on-farm study (not experimental field plots) covering a wide range of

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soil textures, in which contrasts in management between systems might be less pronounced as in filed plot experiments.

Reviewer comment: 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 permebility/gas diffusivity and SOC. I assume the true correlation is between waterfilled porosity and SOC content.

Response: Water-filled porosity (which we see as the complementary of the pore space to air-filled porosity) does not account for pore connectivity/tortuosity (pg2 L19-24). Furthermore, our regression analysis showed that air-filled porosity (and thus water-filled porosity) is less suited as a predictor for soil organic carbon content than gas transport properties (pg9 L19-21, Figure 4, Supplemental Tables S1-S3 and Supplemental Figure S3 and S4).

Reviewer comment: 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

Response: As stated above, numerous previous studies showed that even decreases of just a few percentages of oxygen in soil air might significantly reduce root growth and thus soil organic carbon inputs (pg. 3 L9-12, pg11 L12-16).

Reviewer comment: 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 specultive at this point and not related to results presented in this study.

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Response: We agree that the initial submission of the manuscript contained a number of possibly too speculative statements, which was also remarked by reviewer #2. Following these suggestions we made numerous changes in order to better address possible uncertainties of our study, i.e. by clearly stating that we propose a positive relationship between gas transport capability and soil organic carbon content, which most likely resulted from increased root growth (pg 1 L26-29, pg 10 L4-8, pg11 L10-15, L21-26, pg 12 L22-25). Furthermore, we clearly state on pg12 L28-29 that opposite relationships may occur in different land-use systems and/or climates.

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