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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: All in all, the present study could be a valuable contribution worth being published in soil, provided that multi-collinearity is taken into account and that relations between SOC or Cmic and soil physical parameters are also looked at for the individual management groups. A revised discussion needs to be more conservative. Ideally also more information is provided on C-inputs. The present conclusion heavily depends on the assumption that root C-inputs were lifted by better soil physical traits. But no proof is provided. Also no proof is given that root-C in these systems forms the single most important precursor of SOC. The consequence is that many of the proposed causal relationships are really tentative. This uncertainty needs to be better echoed into the discussion and title. Having said that, the topic is really pertinent, all

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texts are well written and results have been well presented, and all is based on an impressive volume of work.

Response: We appreciate these critical and highly constructive general comments. In accordance to the suggestion, we changed the title of the manuscript to “On-farm study reveals positive relationship between gas transport capacity and organic carbon content in arable soil”. In doing so we believe the mentioned uncertainties (e.g. no data on root biomass) are now better reflected in the title of the manuscript without losing its main finding, which is the positive relationship between soil gas transport capability and soil organic carbon content. Furthermore, we adapted the Abstract following the same line of thoughts, i.e. being more conservative in our conclusions (pg1 L18-30). Regarding the other general comments, please refer to the comments below (i.e. Influence of exogenous inputs of soil organic matter, regressions in individual management groups).

Reviewer comment: The introduction in principle reads well but should be condensed a bit further. To my impression the article really starts with p1L16. The preceding part could in fact be entirely omitted.

Response: We shortened the first part of the introduction (pg2 L2-12) but did not entirely delete it as we believe that a brief general introduction to organic carbon content of arable soils and its relationship to soil management is needed. Furthermore, we addressed in this very first paragraph some of the comments raised by reviewer #1.

Reviewer comment: It would also be better to place the research hypothesis (p3L23) closer to mention of mechanisms that would explain why better aeration should lead to more C in soil.

Response: According to the suggestion, we moved the “aims” of the study closer to the explanation of the mechanisms underlying the relationships between soil gas transport properties, root growth and soil organic carbon content (pg3 L6-27).

Reviewer comment: M&M P4L9 samples were collected in spring: across a single whole season soil environmental conditions could have evolved: e.g. in April drought and in late May a wet period. As a consequence variables like microbial biomass, respiration and soil penetration resistance are also depending on time of sampling. Was the impact of actual sampling date investigated on the studied soil parameters? Or is this effect negligible? I could assume so for microbial biomass given that the substrate-induced respiration method was used. In other words, was the effect of a covariate sample timing required in the ANOVAs?

Response: The soil samples were collected within a period of $\sim$40 days between late April and the end of May (information is now included in the Material and Methods section, pg4 L8). In the study we followed a specific sampling design: The different fields were allocated as triples, which means that one field of each management system (conventional, no-till and organic) were geographically close to each other. Sampling was done following this layout, i.e. one triplet was sampled per day (information is now included in the Material and Methods section, pg4 L9-11). This was done to avoid confounding effects of location/sampling date on the differences between management systems. However, due to this design we are unfortunately not able to disentangle effects of the location from effects of the sampling time with the presented statistical approach (linear mixed model followed by ANCOVA). Nevertheless, we introduced sampling time as an additional predictor variable into the multiple-linear regression models (Eq. 3, pg7 L5-7). This analysis showed that sampling date had no significant influence on soil organic carbon content (pg9 L33-pg10 L1, Supplemental Tables S5-S8).

Reviewer comment: The statistical approach is very clearly described: really helpful. The approach to cover a wide range in soil texture complicates the analysis but on the other hand promotes representativeness of this work. The authors have done well in accounting for variation caused merely by soil texture. The outcome of the ANOVA is also well presented in the figures. One question though: In the ANOVA of non-texture

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variables a factor clay content was included, but interactions with the other factors were disregarded. On what basis did you decide to do so? Could we assume the impact of clay% on SOC, Cmic etc. is independent from depth and management?

Response: This choice was made due to the absence of significant management-depth interactions on clay content (Table 1). A statement addressing this is now included in the Material and Methods section (pg6 L20-22).

Reviewer comment: Also, why clay and not silt or sand%? Please comment.

Response: We chose clay content because it is commonly seen as the textural fraction that is most closely associated with soil structure and soil physical properties as well as with soil organic carbon content. In addition, clay was suitable as it showed the highest variability (expressed as coefficient of variation) among sites. Two sentences motivating this choice are now included in the Material and Methods section (pg6 L19-21).

Reviewer comment: I am missing the point why significance of Eq. 4 is introduced as well. Any relations between SOC content and soil physical variables were already investigated by Eq3. The regression analysis with soil physical traits as dependent variable are apparently redundant.

Response: As suggested we have removed Eq. 4 and all associated statements in the Results and Discussion section.

Reviewer comment: Results: Results have been well presented, just one remark: L11-12 seems to be in contradiction with Fig. 2: total porosity of the subsoil did significantly differ between management systems.

Response: Thank you for this. We have changed the respective statement (pg7 L29-30).

Reviewer comment: Otherwise a well written and clear section. Generally though, no account was made of multi-collinearity among explored predictor variables. In 3.4 the positive relation between clay content, air permeability and gas diffusivity and microbial biomass was discussed. But at the same time we know that SOC content dominantly explains ‘micC’ and ’Resp’ (Table 5). It cannot be excluded that mutual positive correlations exist between 1 SOC and 2 clay content, 3 air permeability or gas diffusivity. From the regression models presented in Table 4 it is then not possible to conclude that air permeability or gas diffusivity have a direct significant impact on micC. Their relation may very well be indirectly manifested through SOC content. More conservative regression models that exclude redundant variables are needed here. In any case the authors should more carefully draw conclusions in this study and leave room for alternative explanations than the currently forwarded main conclusion.

Response: We agree with this comment. Therefore, we removed the regressions formerly presented in Table 4 and present more conservative regression models, i.e. microbial carbon/respiration as a function of soil organic carbon content/microbial carbon. This is mentioned in the Material and Methods section (pg7 L8-13, Eq. 4, Table 5). The results are also presented/discussed in a more conservative way in order to emphasize that soil aeration might have an indirect effect on microbial biomass and activity by increasing soil organic carbon content (pg10 L9-15, pg11 L28-33).

Reviewer comment: Discussion: The current study’s main conclusion, viz. aeration, here represented by gas diffusivity and air permeability, significantly controls SOC levels in soils seems premature. This conclusion is drawn from positive linear relations between SOC level an these soil physical variables based on a set of organic and conventionally managed agricultural fields. SOC levels were significantly larger in topsoil of the fields under organic management vs. under conventional management, as could be expected (only organic nutrient sources, cover crops, ley). Obviously a larger SOC level also increases soil strength and lowers soil bulk density, with then also improved aeration. It is then not warranted to immediately conclude that vice versa improved aeration leads to higher SOC levels. Such could only be said if inputs of exogenous OM was more or less constant (aside C inputs from roots) in the investigates set of fields. Separate regressions for on the one hand CT and NT fields and OR fields on the other also need to be presented. If similar trends are found as in the full (n=30) set then indeed the conclusion seems viable. Looking at Fig. 4, with 4 of the OR fields with highest SOC levels, this may not be the case. The assumption that root-derived C-inputs are superior sources of native SOC in comparison to above-ground plant parts has indeed been demonstrated by several researches. Indeed the so-termed 'relative-contribution factor' of root-C is 2-3x that of above-ground plant parts. But to evaluate if indeed roots form the dominant source of native SOC in the studied fields, the readers need to get more insight into the crop rotations and exogenous OM input management. If in case of OR, exogenous OC input by far exceeds that from roots (more than a factor 2-3) than it seems much less likely that any relation could exist between SOC level and gas diffusivity and air permeability.

Response 1) Exogenous soil organic carbon inputs vs. inputs from roots: We consider this as a very valuable comment. Therefore, we present now data on inputs of soil organic carbon (both crop residues and organic amendments such as slurry, manure and compost) over the last five years before fields were sampled. The calculations are based on information we obtained from the farmers (data was available for 29 of the 30 fields, pg5 L22-31). Interestingly, there was no significant difference ($p > 0.50$, Figure 3) between the three management systems with regard to the amount of exogenous organic carbon input (pg9 L5-6). To evaluate whether exogenous inputs of organic matter influenced soil organic carbon content, we included the total amount of organic carbon input (i.e. sum of residues and amendments) as an additional predictor into the regression models (pg7 L5-7). The results of these regressions are presented as supplementary tables (Supplemental tables S9-S13) and in the Results (pg10 L1-3), Discussion (pg11 L22-24) and Conclusion (pg12 L22-25) sections. In summary, we did not find any significant correlation between exogenous inputs of organic matter and soil organic carbon content (topsoil and subsoil). Hence, we propose that the differences in soil organic carbon content were caused by difference in root derived carbon. We elaborate on that in more detail (and in a more conservative way as in the original submission) in the Results (pg10 L6-8), the Discussion (pg11 L21-26) and the Conclusion (pg12 L22-25) section. Response 2) Regressions for individual management groups: We agree that such an analysis is of value, especially if the aim is to evaluate whether relationships between soil gas transport properties and soil organic carbon content change with soil management. However, in the current study we did not aim to explore this aspect (pg3 L21-27) but to investigate links between soil aeration and soil organic carbon content per-se. Nevertheless, we performed the following additional regression analyses, which are similar to those suggested by reviewer #2: Instead of performing the regressions for the organic management system on the one hand and the no-till and conventional on the other hand, we did the analysis for the no-till on the one hand and the conventional and organic on the other hand. We chose to do so, i) since the predictor variables (i.e. gas diffusivity, air permeability, air-filled porosity and water holding capacity) were mainly affected by tillage rather than organic farming practice (pg9 L24-29, pg10, L30-31, Figure 2) and ii) since the amount of exogenous organic carbon input was not higher in the organic system than in the two systems that also receive mineral fertilizer (pg9 L5-6, Figure 3). These separate regressions are summarized in the Results section (pg9 L24-29) and included in the Supplement (Supplemental Tables S1-S4). More importantly, these regressions support our main conclusion of the study ("positive relationship between soil gas transport capability and soil organic carbon content") since this positive relationship also occurred when looking at tilled and untilled fields separately.

Reviewer comment: No root biomass data were supplied to back p11 L5s sub conclusion.

Response: We unfortunately do not have data on root biomass over the last five years (would be an incredible sampling effort to sample 30 fields at multiple locations for five years). In order to address the comment we rephrased statements on root growth in a way that makes clear that we "propose" or "suggest" a positive relationship between certain soil physical properties, root growth and soil organic carbon content (pg 1 L27-

29, pg 10 L4-8, pg11 L21-26, pg12 L5-7, pg12 L22-25).

Reviewer comment: At the same time the authors best recognize that at present also other views exist: several recent studies have highlighted that the aboveground residues are more important for long-term SOM stabilization (HF-SOM) as compared to belowground. This has been often linked with the relatively high decomposability of aboveground residues which generate more microbial by-products, which are actually the precursor of the long-term stabilized SOM, associated with HF e.g. Cotrufo et al., 2013 (GCB), 2015 (nature geosci); Lavallee et al., 2018 (BG).

Response: Thank you for this remark and the link to the references. We addressed it in the Discussion section (pg11 L 3-8).

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