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

Anonymous Referee #2

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

work.

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. 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.

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

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. 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.

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 re-

searches. 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. No root biomass data were supplied to back p11 L5s sub conclusion.

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).

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