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Interactive comment on "The soil organic carbon stabilization potential of old and new wheat cultivars: a $^{13}CO_2$ labelling study" *by* Marijn Van de Broek et al.

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General Comments

In the present study, the authors report their findings from a 13CO2 pulse labelling experiment on different wheat cultivars grown in lysimeters filled with agricultural soil (surface and subsoil). The main study objective is to assess how the use of more recent wheat cultivars with lower rooting depths and root biomass alters organic carbon inputs into soil compared to older cultivars from the Swiss wheat breeding program. This research subject is important, because a large share of the global agricultural land is allotted to the cultivation of cereal crops, and it is unclear how the use of modern cultivars with altered root traits affect soil organic carbon (SOC) dynamics and consequently SOC stabilisation. Here the authors found no significant effects of different wheat cultivars on SOC in the short term. They conclude that the fate of root biomass after the harvest determines cultivar effects on stabilised SOC pools in the long term. The study is based on a sophisticated methodological approach, including an innovative lysimeter-labelling chamber setup as well as state-of-the-art 13C labelling and analysis techniques. The description of materials and methods used for the study is, in general, detailed enough to follow all steps of the experiment. However, a few things still need clarification (see specific comments). The major limitations of the study are the low number of replicates (probably due to the complex setup) and the fact that root biomass was too low for 13C analysis in many samples. Especially the latter impedes drawing conclusions about the input of plant-derived carbon into soil and its variation between the different cultivars over the growing season. Notably, the authors are well aware of these limitations and discuss them appropriately. The presentation of results is generally OK but should be modified in order to avoid redundancy between figures and tables. E.g. Fig. 1 and Table 1, both are showing the same values and statistics for aboveground biomass. Furthermore, you do not need to repeat values shown in figures and tables in the text body, neither in the results nor in the discussion section. I would also recommend to change Fig. 1 and Fig. 2, not separating between biomass and delta13C, instead showing aboveground biomass together with its delta 13C in Fig. 1 and the root parameters in Fig. 2 (as it is structured in the text body). Regarding Fig. 4 and Table 2, I am missing the statistics. These statistics would be necessary to support some of your interpretations from the discussion part. The discussion itself is a bit lengthy and would profit from some restructuring (see also specific comments). The subsections 4.1 to 4.3 can be shortened, e.g. by excluding the repetition of results and streamlining the remaining text. Maybe it is also better to start the discussion section with the main study object (for non-expert readers), which suddenly comes up in subsection 4.4 now. Another possibility to increase readability would be the use of more active and less passive voice, though this is a matter of taste. Overall, the structure of the manuscript is clear and the language is fine. The authors relate their work to a comprehensive set of up-to-date literature and make the data underlying the results available as supplementary material. However, there are a few things in need of improvement and the manuscript will profit from a revision taking into account the addressed points.

Specific Comments

Line25: I think that “net SOC stabilization” is the wrong term. Stabilization implies a long-term effect, which you did not study here (if there was a difference - what about rhizodeposits degraded and respired by microbes off-season?). Therefore, better use “net carbon rhizodeposition” as in the rest of the manuscript.

Line 85: To my mind, this sentence is unnecessary, because the rationale of the study should be clear from the text above. I suggest starting directly with your research questions and marking them as such.

Line 143: Please indicate the approximate time of day when the labelling was carried out.

Line 146: Was it always the same chamber/cultivar for monitoring CO2 concentrations?

Line 149: Is there an estimate for the CO2 concentration at the end of the two hours?

Line 158: What does “limited amount of samples” mean – only at the end of the experiment (i.e. data shown in Fig. 3)?

Line 175: From my own experience, it is better to analyse soil microbial biomass directly from fresh (unfrozen) soil, because the freezing can increase the amount of carbon found in the non-fumigated fraction (probably cell lysis). However, regarding the delta13C values in comparison to SOC, this does not seem to be a problem here.

Line 211: Did you use the same value of -28 ‰ for aboveground biomass?

Lines 217-218: This sentence is unclear. With “some of the input variables”, do you mean biomass or delta13C?

Line 225: Please explain why you used the Janzen and Bruisma's equation in addition to excess 13C. If I understand it correctly, Fig. 4A shows the summed values for all soil layers as excess 13C according to Eq. 3 and Fig. 4C shows the data for individual soil layers as rhizodeposition C according to Eq. 4. However, the unit in Fig. 4C (g m-2) rather points to excess 13C. This must be clarified.

Line 284: Did you find a significant effect for the three blocks? Why did you use the blocks as fixed effects and not as random effects, i.e. error term, in the ANOVA? Please also report the significance levels for the different statistical tests. In general, I would prefer using the Tukey-HSD test, because it also accounts for multiple comparisons (in particular when depth is added as additional factor).

Lines 296-300: The aboveground biomass values are repeatedly reported in the text, Fig. 1A and Table 1/Table S1. It is sufficient to show the results once, especially since all individual values are available in the supplementary excel file. Remove this redundancy.

Line 325: Interpretations/conclusions do not belong to the results section. Delete this sentence.

Line 341: Note that the soil microbial biomass was higher in Zinal (Fig. S3), so that excess 13C was probably similar to Mont-Calme 268 (Fig. 4C).

Line 357: Do you mean "statistically significant" with "substantially"? Unfortunately, no statistical information is provided in Fig. 4.

Lines 364-367: Please improve the sentence structure.

Line 372: Do you have any explanation for the abrupt increase of Co2 concentrations?

Line 399: How are your results (no differences in root biomass between cultivars) in line the study of Friedli et al. (2019), showing substantially (statistically significant?)

higher root biomass in older cultivars than in more recent ones? That is contradictory!

Line 406: To which species does the root:shoot ratio of 0.14 belongs to, is it an average value?

Lines 429-444: This paragraph reads like an introduction passage. It is better to move it to delete it from the discussion and combine it with overlapping parts of the introduction.

Lines 459-471: The repetition of results should be avoided and the two paragraphs streamlined to 2-3 short sentences.

Line 492: By "assess the effect of wheat cultivars from a century of wheat breeding", do you mean that you assessed the effect of four cultivars representative for changes during a century of wheat breeding?

Line 494: There is no statistical support for this statement, neither for root biomass nor for belowground carbon allocation. In consequence, it is not surprising that you did not find effects on the SOC pool according to the next sentence.

Lines 506-509: This cannot be generalized, because the activity and substrate preference of microbial communities depends on a variety of factors (e.g. Delgado-Baquerizo et al., 2016: https://doi.org/10.1111/1462-2920.13642). In addition, the preference for recent plant-derived substrates or more stable SOM varies with soil depth (Kramer & Gleixner, 2008: https://doi.org/10.1016/j.soilbio.2007.09.016) and the presence/quality of plant residues is known to alter soil microbial communities (e.g. Bai et al, 2016: http://dx.doi.org/10.1016/j.apsoil.2015.09.009). In this sense, the microbial community can be shifted to more fungi and Gram-positive bacteria in the presence of more complex organic compounds derived from root residues.

Line 519: There is no statistically significant difference, only a slight trend.

Technical Corrections

Lines 47-49: Please reformulate the two passages with "is also proposed". This is very repetitive, since the term “has been proposed” is already present in Lines 45-46.

Line 146: Obviously, this should be 40 g and not 40 mg.

Line 179: This sentence fits better in the previous subsection at line 172.

Line 193: Technically, you measured the delta 13C of C-F and C-NF instead of microbial biomass.

Lines 249-250: Separate the “s” and “i-1,i”/”i,i+1” in the formulas (maybe by a semicolon), as it can be confusing otherwise.

Line 350: Replace “showed substantial variation” with “varied”.

Line 381: Include “biomass” (Plant biomass, carbon dynamics. . .).

Line 416 “were respiring CO2 down to greater depths. . .” -> Reformulate.

Line 453: The shown references do not include only the same studies.

Line 485: Twice “assess/ed”

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