

Reviewer 1

Thank you for your careful revisions of the manuscript. I have two main points, followed by line edits for minor revision.

We appreciate the thoughtful and detailed comments outlined here, and have endeavored to address them appropriately.

The hypothesis does not require the analysis of microbial indicators (PLFA) or C composition data (FTIR): "We hypothesized that 170 significant increases in subsoil carbon stocks were associated with the combination of high concentrations of soluble C and nutrients from compost and increased hydraulic transport associated with cover crops." Please include each of these within the hypotheses, objectives, or research questions, otherwise these methods are not justified to be included in the manuscript. Ideally the hypotheses can link the main metrics (C composition, microbial indicators, hydraulic conductivity, nutrient and C at depth within an interactive mechanism or overarching theory - like cascade theory).

We have modified the hypothesis paragraph to better incorporate all measurements made during the experiment. The modified paragraph is below:

The goal of this study was to explore some of the potential mechanisms behind the observed differences in carbon storage in different RR management systems, and to see how these carbon stores have changed after an additional 7 years. In particular, we focus on the role of cover crops in promoting hydraulic conductivity, and how those hydraulic changes impact water, C chemistry, nutrient distribution, microbial biomass and community composition in the subsoil under the addition of additional C (compost) and N (nitrogen fertilizer). We hypothesized that the combination of cover crops and additional C input would result in large amounts of soluble C and nutrients being transported deeper via hydraulic transport, leading to more microbially processed carbon and increased carbon stocks in the subsoil. We also hypothesized that these differences are not due to seasonal variation, and that increased soluble C and nutrient stocks will be consistent at multiple timepoints throughout the year.

The first half of the discussion needs to integrate paragraphs summarizing the literature (more fit for an introduction) with the paragraphs on the current findings. Specific lines within the discussion are indicated below for these edits.

We have reorganized the beginning of each discussion section to explicitly reference the results.

Line edits:

133 Define SOM - first use in the main text here.

Edited

150 - I don't think you necessarily need the cover crop type listed in the intro, merely when you make direct comparisons within the discussion.

Removed cover crop types from the introduction.

185-193 - Experimental design can be above the hypothesis in the introduction.

Modified experimental design paragraph to better highlight the broader questions of the paper, and moved above the hypothesis section.

190 - You can add climate warming "and subsequent soil C losses" to tie this final sentence into the experiment tightly.

Included

233 - Insert space for "2 m".

Included

246 - Merely a suggestion - this information could go into a table to improve accessibility, but is fine as is.

As we already have 9 figures, we would like to avoid including another table if possible.

273 - Define EOC the first time it is mentioned.

Included EOC definition

305 - Add "For each sample 6g of soil" so that the sentence does not start with a number.

Edited

317 - Again, start with "The" before 2018 carbon and nutrient stocks...

Edited

317 - Previously 'C' and 'N' are used for carbon and nitrogen, stay consistent.

Changed all mentions of "carbon" and "nitrogen" to "C" and "N" where appropriate

325 - Is there a need to account for microaggregate-sized sand? (Simply by rinsing a subset of the microaggregate sample through the 250 sieve completely and weighing before and after dry mass.)

We have particle size analysis data for the soils used in this experiment, and they do not show significantly different sand contents between systems. We would also like to note that Wang et. al (2017) found significant differences in microaggregate size fractions in these plots, without accounting for fine sand. While accounting for microaggregate-sized sand may indeed modify the results, we anticipate that this would be a uniform effect across our samples, and would therefore not change the conclusions of the paper.

328 Delete second "PLFA analysis" in the last part of the sentence

Edited

330 Subscript on N₂

Edited

334 Lowercase S on PLFAs

Edited

353 Ensure there is a citation for R here too.

Included citation

373 Stay consistent with "carbon" vs "C" - check throughout

Changed all mentions of "carbon" to "C" in the manuscript where appropriate

390 The first paragraph of the results section is repeated twice.

We are unsure of what precisely this refers to - the first paragraph of the results sections deals with C inputs, while the second deals with N, P and S inputs.

420 These graphs look much better! As a suggestion, generally gray-scale is preferred for accessibility and for readers who may print B+W only. I don't think the treatments (OG, CONV, CONV+WCC require colors).

We have edited all graphs to make them greyscale - thanks for the suggestion!

431 - Typically the alpha level is set prior to the analysis (eg, $p = 0.05$ or 0.1), this allows for consistent interpretation of the results. Here the $\sim 3\text{Mg ha}$ "increase" is not significant at that level, so it should not be reported as an increase unless the p-value is set to above 0.26. Essentially, there was not high enough power to detect if the observed difference is real.

We have modified the wording to clarify the absence of any significant effect here, and reworded the title of Discussion section 4.4 to read "Compost + Cover Crops increased profile C stocks after 25 years, but Cover Crops alone did not"

455 Recommendation: Flip the y axis for Fig. 4 so the soil 'surface' 0 cm is at the top of the graph to make it more intuitive.

We are unsure of what precisely the reviewer means here: this graph shows "Depth Equivalent of Water", not moisture at different depths in the profile. We have modified the caption and label on the graph to attempt to clarify this.

465 Is that averaged across dates?

Yes, this is averaged across dates. We have clarified this in the wording.

Fig 9 a-d is separated by depth rather than date - Is it possible to present the nutrient data in the same way (Fig 6) - by depth rather than date? It seems the nutrient analysis was not repeated measures, but analyzed as an average over time (L 464). To me, it makes more sense for Fig. 6 to also just be an average over the growing season and presented at different depths. The depth patterns are set up as the most important for hypothesis testing in the introduction, and seasonal dynamics were not as important. Alternatively, you could also add an interesting hypothesis about seasonal dynamics.

We have added a hypothesis about seasonal dynamics in the introduction. Since measurements of soluble nutrients are highly variable during the year, we wanted to be sure that our observations of differences between the plots were not just due to conditions at a single timepoint. We hypothesized that the observed differences would persist throughout the growing season, and have added a sentence in the discussion to address this.

Fig 7 B - This is Mineral N, not total N. Please correct throughout the manuscript.

Corrected all graph labels

573-583 - Delete this paragraph or integrate it within the context of your results, rather than summarizing the literature as if for an introduction.

Changed layout of paragraph to begin with results

583 - Fig. 4 is the depth of moisture, not moisture content. If the volumetric moisture content is the important variable, then present that in the text, and put the water depth into supplemental.

We are unsure of what precisely the reviewer means here: this graph shows "Depth Equivalent of Water" which is a measure of moisture content in the profile similar to cm of rainfall. We have modified the caption and label on the graph to attempt to clarify this.

656 - SOC vs soil organic carbon - stay consistent

All "soil organic carbon" terms in the manuscript have been changed to SOC where appropriate

636-649: Cascade theory was introduced already in the introduction, so you can remove those lines here. Also this whole paragraph does not mention findings of this study. Please start the discussion with the present experimental findings and then bring in supporting information from the literature to help create a seamless discussion.

Changed layout of paragraph to begin with results

703 From Fig. 7B it looks like ORG subsoils did not have significantly different mineral N, so please explain how it was a 'higher value in ORG subsoils'. Also, how are you distinguishing higher movement of nutrients at depth from merely lower crop uptake?

We have modified the paragraph to represent the non-significance in mineral N content, and added a sentence referencing previous research at RR showing that cover crop treatments did not have significant effects on P availability or N use efficiency.

730 - Rather than "we attribute" perhaps "we observed/found evidence that the SOC..."

We have modified the sentence to read "We have found evidence that ..."

735 Again, I would change the hypothesis to include something about temporal dynamics, or simply aggregate data across the four dates. Can you explicitly link the seasonal data to the objectives of the experiment?

We have modified the hypotheses to include temporal dynamics, highlighting how variable soluble nutrient measurements are, and the need to measure at multiple timepoints to account for this.

739 - In the response to the review, you stated there were no irrigation events during this study, but here state that nutrient transport depends on irrigation water. Can you please explain the time frame you are referring to here in the discussion?

We are unsure what the reviewer means by no irrigation events during the study - we were unable to find the corresponding comment in our original response. In the previous revision, we included a sentence in the Methods section stating "All plots were irrigated with subsurface drip at the time of sampling, having converted from furrow irrigation to subsurface drip in 2014." We also included estimates of the amount of irrigation water applied to each plot in Supplementary Table A5.

741 The growing season stoichiometry is more suitable than what? Than off-season stoichiometry? Please reword the sentence.

The sentence has been reworded.

757 I would shift away from emphasizing microbial activity (in the first half of the sentence) to reflect the factor that changed during this study "This shift in irrigation and decrease in water inputs potentially increased..."

Edited

761 This is the first mention of tillage within this experiment. Please list in the methods/experimental design.

We have included a brief description of tillage in the methods, and included Supplementary Table A5 briefly outlining practices on the experimental plots for the 2018-2019 year.

764 "In turn, higher transport led to increased C stocks and reduced levels of microbial stress" This was supported by results, but also needs to be part of the hypothesis and objectives of the study to justify the methods used and inclusion of this as a main concluding point.

We have edited the hypotheses to include C stocks and variation in microbial biomass.

Reviewer 2

This manuscript entitled "Synergy between compost and cover crops in a Mediterranean row crop system leads to increased subsoil carbon storage" is a field-based experiment comparing the long-term (25 years) impact of different agricultural management practices on top- versus sub-soil carbon stocks. In addition to carbon stocks, the authors also assessed a variety of soil biological, chemical, and physical parameters to help explain the environmental processes leading to their observed soil carbon stocks results and thus lend support for testing their hypothesis.

Although this is the first time I have reviewed this manuscript, to my knowledge it has been reviewed at least once before, in which extensive edits by multiple reviewers were called for. When comparing the 'response to reviewers' document with the current version of the manuscript, I am happy to see that the authors have very carefully and thoroughly integrated the reviewers' comments into the manuscript, thus clarifying the majority of questions or inconsistencies from the earlier manuscript version. The resulting manuscript is very well-written, and the introduction, methods, results, and discussion are presented clearly and all well-integrated with each other. Below I give some minor suggestions for further improvement.

Thank you!

General comments:

Was it not possible to estimate root biomass and nutrient content in the crop and cover crop roots? This would of course influence overall C stocks but was not mentioned at all in the text.

We do not have measurements of the root biomass and nutrient content of the crop and cover crop roots, as the only measurements made were aboveground biomass. We have added a sentence in the Methods section to clarify that our figures show only aboveground biomass, as well as a sentence in the Discussion to highlight the potential oversight.

What were the differences in crop production between the different systems? Perhaps this could be mentioned as something to take into consideration (i.e. it is of course ideal to improve soil C, but it is also important from an agronomic perspective to ensure appropriate crop yields, which may not be in alignment with increasing soil C).

We have inserted a sentence highlighting the small difference in tomato yields between our systems in the introduction, and added a citation to support this.

Although you state it in the Results section, I suggest also mentioning in the Discussion section that the ORG system also had significantly more C added to the system, as I imagine this could have had an impact on subsoil C stocks as well as the overall nutrient mobility processes.

We have highlighted the larger amount of C added to ORG plots as a reason for increased EOC and total C stocks in sections 4.2 and 4.4.

Fig 1: the individual panels are not labeled a-d as it describes in the caption. I would also suggest arranging them either 2 X 2, or four side-by-side or stacked on top of each other. The current arrangement is a bit off-putting. Also, was there no phosphorus in the cover crop or crop residue biomass?

We have added the requested panel labels, but would like to keep the graph layout as-is due to the common legends for graphs B,C and D

I would suggest integrating Section 3.5 ("Aggregation") into a different section as there is only one sentence describing the results.

We have integrated the Aggregation section of the results into section 3.4.

Fig. 8: the individual panels are not labeled a-b as in the caption. Also, the figure itself is quite blurry. Is there a way to make it more clear (as in Fig. A3)?

We have added panel labels and changed the resolution of the figure to make it clearer.

Table 1: Is this the information shown in Fig. 8? If so, I would suggest only showing Fig. 8 in the main text, and putting this table in the Supplementary Materials.

The raw information used to make this table was indeed taken from the same measurements used to make Figure 8, but this table was added in response to a previous reviewer's comments highlighting the potential issues with visual estimation of spectral subtractions.

We believe that both figures add unique data and should both be included in the main body of the paper: the spectra provide an overall look at the compositional differences with treatment and time, while the table highlights a specific pair of peaks in the spectra that would be hard to otherwise visualize alone.

Specific comments:

L20: I would replace "the increased abundance. . ." with "an increased abundance. . ."

Edited.