SOIL Discuss., referee comment RC1
https://doi.org/10.5194/soil-2021-19-RC1, 2021

Comment on soil-2021-19

Jonathan Sanderman (Referee)

Referee comment on "Synergy between compost and cover crops in a Mediterranean row crop system leads to increased subsoil carbon storage" by Daniel Rath et al., SOIL Discuss., https://doi.org/10.5194/soil-2021-19-RC1, 2021

In this manuscript, the authors present a compelling hypothesis of how compost in combination with winter cover crops can lead to accumulation of aromatic-rich subsurface soil carbon. The hypothesis is complex but plausible whereby cover crop roots improve soil structure/porosity facilitating greater transport of soluble C and nutrients derived from the compost directly to the subsurface where this C can be stabilized. While the hypothesis is compelling, unfortunately, I do not think the authors have collected the right data to test this hypothesis.

Utilizing a long-term field trial should be a great way of trying to address this hypothesis. However, a major limitation of the study is that there is no compost-only treatment, so there is no way to separate the effect of compost alone from the interactive effect of compost and cover crops together. There is nothing the authors can do about this except recognize this as a limitation of the study design.

A major feature of the author's hypothesis is that cover crop roots have created greater porosity that facilitates greater water flow down the soil profile. The data simply do not support this notion. The authors find no difference in saturated hydrologic conductivity at 35 cm (although there was a trend for much greater variability in the compost + cover crop treatment) and no difference in soil aggregates across treatments. The only significant difference was greater water content in the two treatments with cover crops but the authors did not measure bulk density in the 2018 samples and they did not measure porosity so it is difficult to come up with an explanation for this observation.

The next major component of the hypothesis is that compost leads to greater soluble C and N. The authors use salt-extractions of soil samples at four time points during the 2018 season to generate supporting data. Salt-extractable C is an interesting carbon pool (a potentially soluble pool of C) but there is ample evidence that this lab-extracted pool has little relationship to DOC when collected in lysimeters in the field. Without direct collection of DOC diffusing and advecting down the soil profile it is difficult to say whether the differences in the extractable pools are actually leading to more DOC flux to the subsoil under compost addition.

The third component of the hypothesis relates to the preferential partitioning of DOC chemistry down the soil profile. The evidence here is particularly weak. Mid infrared FTIR spectroscopy is not a quantitative analytical tool for determining abundance of specific compounds. If it were, labs wouldn't spend millions of dollars on more precise equipment. FTIR spectroscopy is good for identifying compounds in simple mixtures but not for quantifying their abundance in simple or complex mixtures (and soil is one of the most complex there is). Peak features depending on if they are due to vibrations, wiggles, combinations or overtones all have different relationships between abundance of the specific bonding environment and absorption – basically, you would have to prove that there is a linear relationship between "aromatics" and those two peak features in order to do a spectral subtraction and have any confidence that the difference spectrum represents real differences in chemistry. I also find it problematic that all treatments have showed the same increase in carboxylate functional groups over 25 years – wouldn't we expect the conventional treatment to be more or less at steady state, so we shouldn't see the same changes as seen in the cover crop and compost + cover crop treatments? Lastly, what is the actual magnitude of the "increase" in aromatic features in the compost treatment over the conventional treatment? There are no units on the y-axis. The authors have replicates so they could run statistics to see if this increase was significant.

Finally, the microbial data is not well integrated into the hypothesis. Would lower microbial stress result in greater carbon stabilization via increased carbon-use efficiency or would it result in greater priming and potential loss of older SOM? Regardless of what microbial stress means for carbon cycling, the data were non-significant across treatments. The only significant difference was in Gram+:Gram- ratio but the ecological significance of this difference was not described.

Just to reiterate I think the hypothesis laid out here for subsoil C accumulation under compost and cover crops is entirely plausible but the evidence in this study to support the hypothesis is not particularly strong.