

SOIL Discuss., referee comment RC1
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Comment on soil-2021-41

Anonymous Referee #1

Referee comment on "Long-term impact of cover crop and reduced disturbance tillage on soil pore size distribution and soil water storage" by Samuel N. Araya et al., SOIL Discuss., <https://doi.org/10.5194/soil-2021-41-RC1>, 2021

General comments

This paper presents measurements of topsoil hydraulic properties made in four treatments of a long-term field experiment in California (with/without cover crops, conventional till/no-till) and uses these properties to simulate short-term soil water storage changes following subsurface drip irrigation.

The authors conclude that the no-till + cover crop system improves soil water storage (and presumably also the supply of water to the crop). However, this conclusion does not seem too well supported by the data as many of the differences between the treatments appear to be small. Many times in the paper, the authors report p values >0.05 as being statistically significant, which is not standard practice. I am speculating now, but I can imagine the authors may have been worried that negative results and conclusions would not be considered publishable. In fact, I think that this kind of publication bias may be quite common. If so, it would be unfortunate because it would mean that our consensus view on the efficacy of different management practices to improve soil health could be somewhat biased.

The small number of replicates (4) is an important limitation of this study. Forward modelling based on such limited replication, without the benefit of field monitoring data to calibrate and validate the model is very uncertain. These uncertainties are not discussed by the authors. Field measurements of soil water contents/potentials during and after irrigation would have helped to strengthen the study.

More details on the soil properties and the tillage practices implemented at the site are also needed (points 6-8 under "Specific comments"). The routines in the model related to root water uptake and soil evaporation also need to be better described (see points 10 and 11 under "Specific comments")

The paper is generally well written and easy to read. There are a number of grammatical errors in addition to the ones noted below under "Technical corrections", but these will be easy to correct.

Specific comments

- Line 18, and lines 20-22: these statements appear to be contradictory. First, the authors write that the differences in water storage are marginal, but then they conclude that NT and CC systems improve water retention at the field scale. This should be clarified.
- Line 44: The authors should write this in a less categorical way, by replacing "have been shown" to "may", because it is definitely not always the case that reduced or no-till systems increase carbon sequestration in soil (see Meurer et al 2018). This is probably mostly because under some agro-environmental conditions, crop growth is poorer under no-till (Pittelkow et al., 2015), which reduces the carbon inputs to soil.
- Lines 47-48: Yield certainly is sometimes compromised by no-till (see point 2 above, Pittelkow et al., 2015). Please re-phrase this to acknowledge this fact.
- Line 60: 900 mm annual rainfall?
- Lines 61-62: Perhaps you should explain why no-till can exacerbate compaction problems (because the soil is not loosened, but it is still trafficked) ... and also that this can impact yields negatively (Pittelkow et al., 2015)?
- Lines 78-79: it's only a more descriptive term if no-till really was adopted at the site. This is not fully clear to me. At line 75, you write "reduced disturbance". It's important to be clear and explicit about the tillage system. Is it "reduced tillage" or "no-till"? The tillage operations should be described in more detail for both ST and NT, giving information on the time of year, the implements used, and the depths to which they are operating.
- Lines 81-83: it's important to mention that the tractor traffic was controlled in this way, but what about harvesters? Presumably this kind of traffic was not controlled in the same way?
- Lines 84-87: The authors must give more information on the basic soil properties at the site: at the minimum, information is needed on the particle size distribution and organic carbon contents (the latter specified for each treatment, as they presumably differ after 20+ years).
- Lines 128-129: Why did you calculate unsaturated K at -10 kPa, and not say, at -33 kPa (field capacity)? You should also say how you did this: by fitting to the HYPROP data presumably?
- Line 174: What does "... a radius of maximum uptake intensity at 0 cm" mean? Not all readers will be familiar with this 3D water uptake model, so this should be explained better.
- Lines 175-182: this entire section is unclear to me. The authors mention evapotranspiration, but it's not clear how soil evaporation and transpiration are dealt with individually. For example, the name Feddes is mentioned on line 172, so I presume that the Feddes model is used to calculate actual transpiration from the potential uptake rate, but this should be explicitly stated. But how is soil evaporation

reduced below the potential rate (which I presume is given by equation 4) when the soil surface dries out?

- Line 227 and elsewhere in the paper: the authors refer to results with p values greater than 0.05 as “statistically significant” (or similar phrases). The authors should reserve such a description for results with $p < 0.05$. Instead, write “... a tendency for ..” or something similar.
- Line 237: “... healthy organic matter cycling” is vague. This should be written more specifically.
- Lines 246: Jarvis (2007) did not discuss these processes. It would be better to cite the recent review on soil structure dynamics by Meurer et al (2020) here instead.
- Figure 7. Is the y-axis on the the Kunsat plot correct? 10 to the power of 0.004 is only 1.009. Isn't the variation in K larger than 1 to 1.009 cm/d?

Technical corrections

- Lines 45, 47 and 52: terms like “soil fertility” and “environmental quality” are rather vague. Please replace these with terms specific to what was measured in these studies you cite.
- Line 122: add “of water tensions” after “range”
- Line 124: Write ... “We define field capacity ...” (this definition is only conventional in the U.S., not worldwide)
- Line 125: delete the minus signs prior to 33 and 1500 (you refer to suction)
- Lines 130-131: this can be deleted, as it is defined in connection to equation 1.
- Line 163: delete “that of”
- Line 165: “the van Genuchten-Mualem hydraulic model (van Genuchten, 1980)”
- Line 171: add... “(S_r in in equation 3)” after “root water uptake”
- Line 175: insert “An .. ” before “... atmospheric ..”
- Lines 208-210: this sentence can be deleted (it's repeating the methods). The authors should refer to figure 4 here, but in a different way ... “Figure 4 shows one example of ...”
- Line 227: Replace “A unique ...” by “One ...”
- Lines 238-239: this is unclear. Is there text missing here?
- Line 260: section 0?
- Line 327: insert “of drainage” after “days”
- Line 364: replace “steady-state” by “equilibrium”

References

Meurer, K., Haddaway, N., Bolinder, M., Kätterer, T. 2018. Tillage intensity affects total SOC stocks in boreo-temperate regions only in the topsoil - a systematic review using an ESM approach. *Earth-Science Reviews*, 177, 613-622.

Meurer, K., et al. 2020. A framework for modelling soil structure dynamics induced by biological activity. *Global Change Biology*, 26, 5382–5403.

Pittelkow, C., Linqvist, B., Lundy, M., Liang, X., van Groenigen, K., Lee, J., van Gestel, N., Six, J., Venterea, R., van Kessel, C. 2015. When does no-till yield more? A global meta-analysis. *Field Crops Research*, 183, 156-168.