Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
https://doi.org/10.5194/hess-2021-242-RC2, 2021

Comment on hess-2021-242

Anonymous Referee #2

Referee comment on "The impact of soil development, rainfall intensity and vegetation complexity on subsurface flow paths along a glacial chronosequence of 10 millennia" by Anne Hartmann et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-242-RC2, 2021

Hartmann et al. present a generally interesting study on infiltration capacities across a moraine chronosequence, where each chronosequence is divided into three levels of vegetation cover complexity and receives three different water application intensities, resulting in 36 different water applications. However, the experiment appears to be (more or less) a replica of a previous study (Hartmann et al. 2020a&b), with the main difference apparently being the parent material, which is calcareous in this manuscript and siliceous in the previous ones, and an apparent focus on vegetation and rainfall intensity influences. Even some of the figures are largely identical. It is not entirely clear to me what the new contribution of this manuscript is over the previously published study.

Additionally, I do have some concerns with the general study layout and possible interpretations. Each plot is divided into three 50cm wide zones with different rainfall application intensities. These zones are not physically separated from each other and to prevent interaction during application in one zone, the remaining two are covered. This still leaves room for interaction near the zone boundaries where water can be drawn laterally into the drier soil of a neighboring zone. The authors acknowledge as well (see below) that overland flow on some plots might have infiltrated near the zone boundaries, leading to increased infiltration there.

Then there is the question of the ages in the chronosequence. The two younger sites are 110 and 160 years old. 50 years difference is not much in a soil chronosequence, especially considering that the other soils are 4900 und 13000 Jahre alt. Unless I missed it, I did not see an explanation of what the authors expect in those 50 years to have happened to the soil.

If this were the authors' sole publication on the topic, I would probably just consider major revisions (i.e., shortening and some restructuring). Given the other two publications, I am having difficulty seeing the novelty in this manuscript, though, and am unfortunately leaning toward rejection.

Some additional comments:

Moraines are a special type of cover and pedogenesis. Can you hypothesize what can be expected in soils that formed not from direct glacial processes?

Fig 5: Are the figures the mean of the five excavated profiles?

P21 L10-11: Is this purely based on the parent material or maybe also a function of landscape position, e.g., aspect, slope, etc.?

P22 L26-28: For the sake of comparability, would the "finger flow and macropore flow (high interaction)" class be classified as macropore flow in the previous study? (only based on patterns, even in the absence of actual macropores)

P22 L30-32: Given that the top layers contain hydrophobic material in both studies and no overland flow is observed, wouldn't that suggest that most water should make it through this hydrophobic layer?

P26 L18-19: This raises a question about the experiment setup. If I understand correctly, the zones for different application intensities were neither separated by a non-irrigated space in between, not by some barrier installed into the soil profile that could have prevented surface flow onto the adjacent zone? If this is the case, couldn't it be possible that the areas where the transition from one zone to the next happens simply receive more water than the rest of the zones? If deeper infiltration is observed below the transitions, that could be a result of more water infiltrating and thus being able to reach greater depths.