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The Cryosphere Discussions

Interactive comment on "Soil infiltration characteristics and pore distribution under freezing-thawing conditions" *by* Ruiqi Jiang et al.

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Summary and General Comments

The authors have presented an experimental study on the effects of freezing temperatures and one freeze-thaw cycle on soil infiltrability. Using a relatively simple laboratory experimental set-up, the authors subjected repacked soil samples to repeated tension infiltration cycles at varied temperatures and thus frozen/unfrozen states. Results showed that all samples, for the same total soil moisture (I assume, as the authors have not shown the data), exhibit a decline in unsaturated hydraulic conductivity with decreasing temperature, however only modest decreases were observed in samples once frozen. Using multiple applied tensions allowed the estimation of the maximum

pore size contributing to flow. Similar to previous work, the authors found that 'macropores' occupied a small portion of the total pore-volume but contributed disproportionately to flow... very nice to show experimentally that this still occurs under frozen conditions. Interestingly the authors found that the contribution of these larger pores to flow decreased when frozen, and the authors invoked soil structural changes during freezing as the mechanism to explain observations.

The paper is, for the most part, well written. Some sentences are a bit awkward, but the scientific understanding of the authors is clear and insight is still easy to comprehend. The rationale for the study and the methods are explained very well. The results are clear, however some discussion of the underlying mechanisms for the observations need clarifying and further refinement. The authors do not present some experimental data that are crucial to understanding their observations, like soil porosity, measured pre-freezing soil moisture content, and soil-frost during infiltration events. I am also very worried that the soil structure changes observed may be an artefact of the repacking of the sample before undergoing its first freezing treatment. However, the results and other aspects of the discussion are still a solid contribution, and this work should be published. However, there are some important issues that need to be addressed before I can recommend publication.

Specific Comments

An assumption underpinning the authors' tension infiltration analysis is the assumption that larger pores only flow fully saturated (no air-water interface inside the pore) and excludes the formation of an air-water interface with flowing water in larger pores. Recent work has shown this flow mode does indeed occur (see the multitude of works by Drs. John Nimmo and Peter Germann). It would be nice for the authors to acknowledge these limitations in their work.

What was the pre-freezing soil moisture content of the samples? You should show this data.

What was the porosity of the samples? If the pre-freezing volumetric moisture content was 0.3 as the authors suggest (should state more explicitly), then when frozen that will result in relatively high content if the soil porosity is say. . . 0.4 to 0.45. The authors need to clarify and discuss this.

How was the pre-freezing moisture content held consistent between samples, and after BF tests?

In unfrozen tension infiltrometer experiments, the soil moisture is assumed to be that imposed by the applied tension. If samples were frozen before infiltrometer experiments, then is it assumed that the applied tension then only affects the pores that are active during infiltration?

How was the pre-freeing water content controlled?

L51: Inappropriate reference, the review paper of Jarvis (2016) hardly mentions frozen soil dynamics, other than we do not understand it enough.

L56: 'characterization of freezing-thawing soil infiltration' sounds awkward. Do you mean infiltration into freezing/thawing soils?

L60: Daniel et al (1997) should be Stadler et al. (1997). Could also cite some other field studies on frozen soil infiltration and deeper soil percolation and refreezing effects, such as Hayashi et al (2003) and Mohammed et al. (2019).

L71-73: Zhao et al. (2013) did not introduce the 'impedance concept', it was proposed far earlier, at least as early as Jame and Norum (1980).

L75: 'results in hydraulic conductivity estimation'. . . confusing, can the authors clarify?

L89: What does the authors mean by 'freezing profiles'? Do you mean the soil freezing characteristic?

L152: Authors should state clearly that pure water was used as the infiltration solution for the unfrozen experiments. Also were the samples gravity drained after the

unfrozen test? Were the samples adjusted to ensure consistent pre-freezing soil moisture among samples?

L279: What do the authors mean by saturated water content of the frozen soil? Weren't all the samples unsaturated, so how then can there be a saturated water content?

L282-289: Was there any correlation between the amount of decrease in hydraulic conductivity from -5 °C to -10 °C and clay content or organic matter content among the soil samples?

L322-341: This is fascinating, and in my opinion, is the most novel part of this study. But this is conjecture, and there are quantitative ways to examine soil structure before and after freezing, see for example Holten et al. (2018) and Ding et al. (2019), who actually apply geophysical imaging techniques to quantity pore structure in relation to frozen soil infiltration.

L353: I agree, but you need to show those water and ice contents of the soil samples.

L368-372: Not sure I agree with this statement, as your data contradicts it, and at atmospheric pressure, air-filled macropores will conduct most water, regardless of antecedent moisture.

L376-403: I have a few issues with the discussion in this section, mostly because of a point that the authors themselves bring up. . . that these experiments were performed on repacked, air-dried samples. So, although they cite other studies that show that macropores may still play a role in re-packed soil samples, they did not show so in their own data. Also, the notion that macroporosity is decreased after freezing goes against other experimental studies that explicitly investigate the effect of multiple freeze-thaw cycles on soil structure (Ding et al., 2019). This may be an artifact of the fact that this was the first freeze-thaw cycle after the sample was repacked.

Additional References:

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