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Discussions

Interactive comment on "Effects of preferential flow on snowmelt partitioning and groundwater recharge in frozen soils" *by* Aaron A. Mohammed et al.

Anonymous Referee #2

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The manuscript of Mohammed at al. describes a field study of winter time infiltration in a cold region environment in the Canadian prairies where spring snowmelt and mid-winter melt events can have an important contribution to groundwater recharge. They used a sensor setup at three sites, each with two installed soil pits, including measurements of soil moisture, soil temperature and groundwater head. Furthermore, snow surveys were conducted and ponding levels in the depressions were recorded. The authors observed high amounts of infiltration during times when the soil was still frozen and corresponding increases in soil moisture content, some below the frost layer or non-sequential sensor reactions. Additionally, the recession rate of the water in the depression indicated infiltration into the frozen soils. The authors concluded that pref-

erential flow in previous air-filled macropores are the dominant mechanism that allows water under frozen soil conditions to infiltrate. Increased runoff ratios with multiple midwinter melt events indicate refreezing of the infiltrating water and increased surface runoff. A time delay of snowmelt and ponding of water in the depressions was interpreted as lateral shallow subsurface flow into the depressions.

The study faces a highly relevant topic since many studies showed the reduction in hydraulic conductivities of the soil matrix under frozen conditions (e.g. Kane & Stein 1983, doi:10.1029/WR019i006p01547), but others observed that frozen soil still allows water to infiltrate (e.g. van der Kamp, doi:10.1002/hyp.1157). Frozen soil infiltration is a complex topic which includes the heterogeneity of soil flow processes as well as the effect of the soil thermal state and phase change of water. The role of preferential flow in frozen soil infiltration had become more attention in the last years (e.g. Holten et al. 2019, doi:10.2136/vzj2018.11.0201; Demand et al. 2019, doi:10.2136/vzj2018.08.0147; Watanabe & Kugisaki 2016, doi:10.1002/hyp.10939), studies which clearly show that frozen soil should not be treated as impermeable layers. How preferential flow in frozen soils can influences the magnitudes of water balance components over a winter season with multiple snowmelt events on a larger scale (>plot scale) is unclear till now. Therefore, the study of Mohammed et al. gives interesting insights and should be published, but some revisions are needed.

The study fits into the scope of the journal, is well written with a good introduction and clear objectives. The manuscript cites the most relevant literature and has appropriate methods for studying the phenomenon of frozen soil infiltration. I agree with the comments of Referee 1 (Andrew Ireson) and have some additional general comments mainly regarding the length, structure and argumentation of the discussion.

I suggest to restructure and shorten section 5.1 and 5.2 of the discussion. It is rather long for the main findings of the study and repeats many points. For example P10 L1-26 discusses preferential flow for the argumentation of snowmelt partitioning. However, preferential flow is again discussed in a similar way on P11 L20 - P12 L19. I suggest to summarize these parts. Furthermore, I suggest to move the Table 3 and arguments on P11 L20-24 to the results (as it also reads like results). Table 3 is not mentioned in the results and the authors already discuss the relevance of preferential flow without pointing out its evidence in the results. Additionally, the authors should give some more information on the soil properties (if possible). Especially porosity seems to be important in the context of the study, because air-filled porosity is used for the argumentation of preferential flow. E.g. antecedent soil moisture in relation to porosity can be used as an estimate of air-filled porosity available for infiltration.

Please make sure that you be consistent with the description in the results. For example you do not mention MW2 for the Spyhill Upland. Furthermore, sometimes it is not completely clear if you still talk about the upland or the depression (e.g. P6 L27-29).

Specific comments:

P3 L16: Change "…of the region surrounding the study sites …" to "… of the study sites…"

P4 L13: Please specify why it was not always possible to use pressure transducers.

P5 L10-11: How many soil cores were taken and at which depth? I think you first mention this in the results. Why has the Ksat using a permeameter been only determined for this site? Specify that all Ksat measures were performed for unfrozen soil.

P5 L17-18: The slug tests were done for all sites? Please specify this.

P5 L24: Please clarify that even if Ro is underestimated (P4 L23-24) the catchment wide infiltration rate I is correct. An underestimated Ro leads to an overestimation of I at the uplands, but an underestimation of I in the depressions. From the pure equation one can think that the calculated I also contains the error resulting from the R0 estimation (hence I would be too high).

P6 L15 and Table 2: Be more consistent for Stauffer MW1 and use "-" for all components or leave the event out since it was not calculated.

P7 L5-6: Again, I think how you calculated Ksat and that the permeameter tend to underestimate Ksat (with a reference) should be mentioned in the methods.

P7 L22 and Table 2: Why did Triple G had no change of SWE during the spring melt event?

P8 L8-11: Be careful with the comparison of Ksat and recession rates, since a unit gradient assumption is questionable at Triple G with 50 cm ponding head.

P9 L23-28: The runoff ratios also depend on the snowmelt rates observed during a certain event or year. For a comparison of different sites, events or years it is important to mention this.

P10 L16: Delete "subsurface" in "subsurface infiltration/refreezing"

P10 L28: How can you be sure that these lateral pathways are preferential pathways?

P11 L28: Maybe Graham & Lin (2011) (doi: 10.2136/vzj2010.0119) is a better reference.

P12 L10-11: Why do you know that infiltration was limited by the hydraulic conductivity of the zone beneath the frozen layer and not by the frozen layer itself? You do not know the saturated hydraulic conductivity of the frozen layer and from the water level recessions you can just estimate an integrated Ksat (with unknown extend) by assuming a unit gradient (what's not always the case).

P12 L15-16 I would add a reference here, e.g. Schwen et al. (2011) (doi:10.1016/j.still.2011.02.005)

P12 L30-31 I would argue that the infiltration is rather dependent on the amount of connected and air-filled macropores in the frozen layer than on the infiltration rate of the subsoil. Without connected macropores the infiltration rate of the topsoil would be much lower since only the frozen soil matrix would conduct water and hence it would take the water a long time to even reach the subsoil.

P13 L31: Change to “. . . during midwinter snowmelt enhanced surface runoff generation. . .”

P14 L23: Data availability: Delete “and”. Furthermore, there is no Table in the Supplements.

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