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Hydrology and Earth System Sciences Discussions

Interactive comment on "Evidence for enhanced infiltration of ion load during snowmelt" *by* G. Lilbæk and J. W. Pomeroy

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First, we thank the reviewers for taking their time to comment on this paper. Each of their comments has been addressed. Below follows a list describing the responses.

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2-1 I found that the title of the manuscript is somewhat misleading. The readers would expect some field evidence by this title. I suggest to add "Laboratory" in front of the "Evidence".

We agree and have corrected the title

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1-1. The term 'enhanced infiltration' is not appropriate for the process outlined here. 'enhanced ionic infiltration' is perhaps more appropriate as it is only the ion load, not the water mass, that is enhanced. This of course is reflected in the title. In essence, this is just the extra ion concentration that deviates from the product of mean concentration and average infiltration rate due to preferential elution.

We agree and have made corrections throughout the text

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1-2. The pathways of infiltration/percolation and runoff described here are largely associated with cold regions. More clarity is required. Is not infiltration into organic soils infiltration? Does enhanced infiltration not occur in this circumstance? If not, why not? There needs to be more detail here on the factors that control infiltration into frozen soil.

The described pathways are not considered to be for cold regions only – the intent was to make the description general as enhanced ion infiltration is believed to take place whenever partitioning of water occurs at the surface. We feel, we have clearly stated throughout the text that infiltration to the organic layer is infiltration (i.e. p. 2 ln. 13- and p. 3 ln 10-). Regarding whether enhanced infiltration occurs for organic layers; yes, we believe it will take place whenever partitioning of water occurs. Clarification has been added to the text on p. 5 ln 16-. Regarding the need for more details on the factors that influence infiltration into frozen soil, we noted that this is opposite the request from the other reviewer (below). However, we have tried to accommodate both views and made corrections throughout the text.

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2-2 The introduction part could be more focused. A considerable portion of the text in this section are devoted to describe the factors that influence the infiltrations into frozen

HESSD

7, C1057–C1061, 2010

soils (e.g. line 14 page 1433 to line 25 page 1434), such as macropores, basal ice, migrations of moisture to frozen front, etc., yet there were very little statements that relates those factors to the "enhanced infiltration" phenomenon, which is the main subject of this study. Further more, all those factors were not presented in the experiments of this study. I suggest reducing the texts about the non-essential factors in "Introduction", but address them BRIEFLY in "Discussion". Some other questions I would like to know in "Introduction" are: (i) is this "enhanced infiltration" unique in frozen soil or it could occur in unfrozen soil as well? (ii) what are the major similarities and discrepancies between the snowmelt infiltration and that of the experiments in this study?

Some reductions in the introduction part have taken place in order to accommodate both reviewers (see above). Relation of the described factors to enhanced ion infiltration has taken place throughout the text – e.g. p. 2 ln. 24- and p. 3, ln 1-. The questions that were of interest to the reviewer have been incorporated into the text – e.g. p. 5, ln. 16-21. We did not find the second question (ii) appropriate to be commented upon in the introduction as it relate to the setup of the experiments. Instead we included a section in the discussion part to address this – p. 11, ln. 13-.

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1-5. p 1433 Line 21 - the word 'huge' is colloquial and should be replaced.

Corrected to 'significant' – see text p.1, ln. 30

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1-3. You mention (and support with the Tao and Gray reference) that infiltration is least sensitive to frozen soil temperature. I agree that this is likely true in the range of temperatures that you have tested in this study. If soil temperatures are very cold (perhaps <-7oC), latent heat and re-freezing become extremely important, particularly in creating impermeable barriers at depth. Infiltration studies conducted into frozen soils high arctic environments show very different patterns and results than more southerly frozen soils. Mentioning this would be appropriate. I believe that your temperature range is too small to make any definitive statements about its affect on infiltration.

We recognize that the temperature may influence infiltration in permafrost areas. However, this research is looking at infiltration into seasonally frozen soils; thus, we have made clarification to the text in order to address this.

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1-4. What are light textured soils? (p1434 Line 20).

Definitions have been added – see text p.3, ln 24.

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1-6. What role does the speed of freezing have on your results and the migration of water, if any? It is a neglected point.

We believe we commented on this on p 7, ln 23-.

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1-7. Is there a reference to confirm that the maximum 10 mm head is similar to field conditions?

The statement is based on personal observations. Clarification has been made to the text on p. 8, ln 11

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2-3. Lines 9-20 Page 1443. I suggest deleting or reducing those discussions for general frozen soil infiltration so that it could be more focused on “enhanced infiltration of ion loads”.

A reduction of the text has been made – see text p.11-12

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HESSD

7, C1057–C1061, 2010

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1-8. I appreciate the discussion of the solute effect on freezing points in the discussion. I believe that this should also be mentioned in the introduction as well as it was a question I kept asking myself until I finally reached the discussion.

We agree and have added a section to the introduction – please see text p.3, ln 30-

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2-4. Lines 3-5 page 1446 and in Fig. 5 (Page 1457): I am not very clear about the term “volume of liquid water” or “percentage of initial water volume”. Is it just the initial volumetric soil water content or is it related to the added water? Please clarify.

This part of the discussion is related only to the solutions added to the soil surface. Further clarification has been added – see text p. 13, ln 29-

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2-5. Lines 12-14 page 1446: if I understood correctly, the 0.03-0.6 _C freezing point depression is only for the saturated condition (Fig. 5). As showed in Fig. 5, the freezing point could reach -6 _C for dry soil, which means the dry soil will not freeze at all in this experiment (about-1 - -2 _C, table 1). If this is the case, it will be a big discrepancy between this experiment and the field condition. Please clarify.

The freezing point depression does not refer to a specific soil but rather the solution. Thus, figure 5 shows that in the case where a solution with a high concentration infiltrates the soil a small amount of liquid water will still be present allowing further infiltration to occur. Clarification of this has been made to the text as it complements the other reviewer's comments too – please see p. 13, ln 29-

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 1431, 2010.