We thank Dr. Andrew Ireson for his positive review and constructive suggestions, which allowed us to improve the clarity of the manuscript. In the following section, reviewer comments are in bold, author responses in regular font, and changes made to the text are in italics.

Authors response to specific comments:

P. 1, L. 15: "the role of shallow subsurface flow" - I'm being slightly pedantic here, but this is ambiguous language... say explicitly what "the role" is - e.g. that there is a shallow lateral subsurface transmission pathway through the frozen soil, from uplands to depressions... if that's what is meant? Maybe appropriate to use the term "interflow".

We acknowledge this statement was a bit ambiguous. We agree that a more explicit statement is needed and have included the suggested wording of 'a shallow lateral subsurface transmission pathway through the frozen soil' and also included the term 'interflow'. The sentence has been modified to:

At one site, time lags of up to 3 days between snowcover depletion on uplands and ponding in depressions demonstrated the role of a shallow subsurface pathway or interflow through frozen soil in routing snowmelt from uplands to depressions.

P. 1, L. 17: "before ground thaw" - do you mean total thaw, or the commencement of thaw? This could be stated more clearly and precisely.

We agree and have rephrased to 'complete ground thaw' to be more precise.

P. 2, L. 22: It could be appropriate here to note that zero-till cropping, which is in widespread usage for the past maybe 20 years in the prairies, might also allow macropores to be preserved.

We agree and have added a sentence on this.

'*… have a higher frozen soil infiltrability due to the presence and development of a macropore network, in comparison to tilled croplands, where annual cultivation breaks up the macropore network near surface (van der Kamp et al., 2003). <u>In addition, zero-tillage cropping, which has been increasingly adopted in the Prairies over the past several decades, may also allow macropore networks to be preserved (Tiessen et al., 2010)</u>*.'

Additional reference: Tiessen, K.H.D., Elliott, J.A., Yarotski, J., Lobb, D.A., Flaten, D.N. and Glozier, N.E. 2010. Conventional and conservation tillage: Influence on seasonal runoff, sediment, and nutrient losses in the Canadian prairies. *Journal of Environmental Quality* **39**:964-980.

P. 4, L. 2: confusing - should "infiltrates within" be "runs off into and then infiltrates beneath"? I think the point that the pond water level rises are not corrected for the volume of infiltration below the pond during the period of runoff. It would seem reasonable to ignore this likely small error (as the authors have done).

We have made the change as per Dr. Ireson's suggestion. We have also stated more explicitly that the pond level rises are not corrected for the volume lost to infiltration below the pond. The sentence has been modified to:

'*This method underestimates runoff as it does not consider the volume of water that initially runs off into and then infiltrates beneath the depression prior to observable ponding (Hayashi et al., 2003).*'

P. 4, L. 27: It's important to say whether or not the soil pit was installed below the depression or adjacent to the depression. This is critical to the interpretation of pond water recessions juxtaposed against the soil temperatures. This is unclear to me from the text and from Figure 1. Cross-sections in Figure 1 would be extremely helpful to interpret where the measurements are taken from, including piezometer depths.

We agree this is an important point. We have added text to clarify that the soil pit was installed directly below the lowest surface elevation point in the depression. We have also added a cross-section as requested. To address this concern and those of Reviewer 2 (regarding providing more subsurface information), we have split Figure 1 into two separate figures with additional information. The new Figure 1 has the location of the study region, and soil and sediment cover for the region. The new Figure 2 provides the plan view map of the sites along with a cross-section showing subsurface instrumentation. We have also included a new figure with depth-K_{sat} profiles for the three sites.

P 6., L. 31: Your data in Figure 2 and 3 show the water content responding to the Spring melt event before the temperature responds. Why?

We thank Dr. Ireson for mentioning this issue. During the Spring melt event, soil water contents increase before the soil temperatures respond (at the same depths) due to the 'zero-degree' curtain effect, in which latent heat transfer prevents soil temperatures from rising above 0 °C until pore-ice has completely melted. As such, a water content response while temperature at the same depth remains at 0 °C indicates porewater phase change is occurring. In other words, soil frost is present and likely thawing. We have added discussion of this point to the text.

P. 6, L. 32: at Stauffer the increase in RR between MW3 and spring (31 and 33) seems negligible and well within likely error bounds - this point should be acknowledged. The increases are far more convincing at Triple G and Spyhill and maybe there is a reason for that?

We agree and acknowledge that the increase in RR at Stauffer between MW3 and Spring is considered negligible compared to Triple G and Spyhill and have made a statement acknowledging this. We are not entirely certain what the cause of this discrepancy is, but we can speculate that one possible reason is that soils at SE2 have more sand content than the other two sites which are more clay-rich. Thus, in addition to macropore flow, enhanced matrix flow and/or imbibition from macropores to the matrix in frozen soil at SE2 maybe allow more faster drainage to the subsoil during snowmelt and keep the near surface soil relatively permeable prior the next melt event. Another possible explanation is that SE2 may have been subject to some overflow (i.e. fill and spill) to the north of the depression. The relatively stable RR values for SE2 over MW2 and Spring could also be a combination of both factors (sandy soil and some overflow potential).

P. 7, L. 26: This paragraph describes the data in Figure 4, but studying Figure 4 it does not appear correct in a number of places - specifically the first and third sentences.

We thank Dr. Ireson for bring up these discrepancies. We assume that the mistake in the first sentence is that soil moisture increased slightly at all depths 0.2 to 0.8 m, but that only the 0.2 m sensor rose above its pre-freezing value. The sentence has been modified to correct this:

Liquid soil moisture at depths 0.2-0.8 m under the depression gradually increased between events MW1 and MW2 (Fig. 4d) along with a slight response in groundwater level in the piezometer, which was dry prior to MW1 (Fig. 4e), indicating some infiltration had taken place.

We assume that the discrepancy in the third sentence is that the soil moisture sensors at 0.2 m does actually show a gradual response after MW3, suggesting that they're might be some infiltration taking place to that depth. The sentence has been modified to correct this:

After MW3, liquid soil moisture at 0.2 m increased gradually, but no event-specific changes in liquid soil moisture and temperature occurred below that depth (Fig. 4d) despite the rise in the pond level (Fig. 4b). This suggests little infiltration in the depression occurred during this event due to prior refreezing of meltwater in the soil profile following MW2.