Author response to comments from Dr. Spence for HESS manuscript "Controls on groundwater response and runoff source area dynamics in a snowmelt-dominated montane catchment" [Paper #: hessd-10-2549-2013]

Dear Dr. Spence,

We would like to extend a special thank you for taking the time to voluntarily review our original manuscript submission under your own initiative. You raised several thoughtful issues that will result in a stronger submission. Please find below a list of responses to your comments. We hope our responses satisfy the spirit and intent of your remarks.

Sincerely,

Russell Smith

<u>Dr. Spence's comments</u>

Several theories were tested; perhaps too many for a single paper. Perhaps addressing fewer theories, or combining some of the hypotheses would have led to a more succinct paper. For example, if I understand correctly, hypothesis #2 is testing whether or not groundwater tends to move vertically and why, while hypothesis #4 tests why water would move horizontally. I'm not sure competing hypotheses were needed.

- We agree with the point that the quantity of hypotheses is too much for the reader to digest. We will modify the paragraph to focus on the study objectives and provide a brief introduction of the study approach. We will incorporate the ideas behind the hypotheses in the conclusions.

I somewhat disagree that what was observed in this catchment is analogous to the variable source area (page 2576). The only similarity the results have with the variable source area concept is that the "source areas" may have been variable, but the predominant runoff generation processes that create the documented variability are very different than those that produced the responses Hewlett and Hibbert documented.

- Our study results suggest that expansion and contraction of groundwater response areas occurs during peak flow and post peak flow periods, and the corresponding spatial patterns of response are controlled largely by topographic factors during these periods. These points are broadly consistent with the variable source area concept. However, our results also suggest that other factors control the patterns of groundwater response during early spring freshet periods, which is not consistent with the processes documented by Hewlett and Hibbert. We will revise the text to clarify which findings are generally consistent with the variable source area concept.

The results also imply that landscape position, or topology, is crucial. I suggest the authors read and include ideas from Spence and Woo (2003), Buttle (2006) and DeBeer and Pomeroy (2010) that might help interpret what was observed.

- Agreed. Thank you for the references. We will incorporate their findings where appropriate.

Some minor suggestions could be to

1) Improve the figures that are far too small to read (e.g., 7 and 8).

- Agreed. We originally had Figure 8 split into upper and lower halves, but they were combined during the publication process. We'll return to the original format, and revise other figures accordingly.

2) Change the model development approach to be more like a split sample. In Section 3.2; the authors state that observations of unresponsive sites were consistent with model results. Of course they would be, because the authors used all the observations to derive the model. Would a more appropriate course of action been to take half the wells, build the model, and then test on the other half?

- The study was an empirical analysis focused on testing hypotheses about spatial controls on groundwater response, rather than a predictive analysis. As a result, a formal test of the predictive power of the OLR models was not considered necessary. Moreover, the sample size was too small to split the dataset for verification, as logistic regression requires much larger sample sizes than ordinary regression. Our comment that observations of unresponsive sites were consistent with model results was simply to indicate that the raw data corroborate the results of the analyses, as it's not uncommon for statistical models to misrepresent real processes for a variety of reasons.

3) Use a common depth from the surface in deriving metrics of response timing, duration and occurrence. I need to put more thought into this, but that may have made the observations more comparable because the wells were driven to different depths.

- That's an important point and one that we gave extensive consideration. While this suggestion makes sense generally, that approach would have been problematic because of the large ranges in well depths and maximum water table levels measured in the wells. Both factors were governed by the soil conditions, with the latter being influenced also by the local runoff processes. For any given reference soil depth, many sites would have been excluded from the sample population or would have had zero values due to wells being too shallow and due to the observed water tables (for the particular period of interest) being too deep. Combining these factors with the highly transient or unresponsive behaviour of many wells, the sample of observed responses would have been quite small. Given that statistical approaches utilizing ordered classes (which was necessary due to data censoring) require much larger sample sizes than approaches utilizing parametric data (e.g. ordinary regression), the statistical power for investigating the dataset would have been overly limiting. In response to this concern, we decided to incorporate well depth in the statistical models to test for its influence, as well as numerous soil parameters, including hydrologic conductivity profile data. Given that the average well depth for unresponsive sites was greater than that for responsive sites, it is apparent that other factors (e.g. soils, topography, vegetation) determined the occurrence, duration, and timing of groundwater responses, not well depth.

4) Be more specific when on Page 2573 the authors imply that a basin needs to be in a hydrological homogeneous state (e.g., wet everywhere?) in order for topography to be a dominant controlling factor.

- We will revise the text to state that lateral controls begin to dominate the persistence of groundwater response "once snowmelt expands throughout the catchment and most of the catchment is hydrologically active, spanning large gradients in insolation, forest cover, and topography".

5) On Page 2575, explain what is meant by "deep disconnected flows"? Disconnected from what, yet presumably flowing to somewhere?

- By "disconnected", we mean flowing through slow runoff response pathways toward the stream channel, such that the flows likely would not contribute to streamflow during the event. We will specify this point.

6) More importantly, the authors do take some liberties and make several quantum leaps in their arguments that should be explicitly addressed. For instance,

a) What is the process by which thinner trees increase the duration of groundwater response (Table 4, Figure 5 and Page 2568)?

- We believe that maximum tree diameter was an important variable in the models (and consistently explained more variance in the models than other forest cover metrics) due to the disproportionate influence of large diameter trees (and their relatively large crowns) on snowpack shading and evapotranspiration compared to small diameter trees. We will specify this point.

b) How does insolation increase the annual duration of groundwater response?

- Good question. A key factor here is that the effect of insolation on the annual response is positive on high slope gradient sites, but negative on low slope gradient sites (see figure 5). This finding is similar for the occurrence of response. We interpret the positive effect to be related to relatively high snowmelt intensities on steep gradient, high insolation sites resulting in a greater occurrence of percolation-excess processes and, thus, more prolonged responses.

c) Most importantly, while the authors did an excellent job investigating groundwater response, I'm not sure the authors can claim to have analyzed "runoff source areas" as they suggest in the title. The wells used may not necessarily have been deep enough to capture all the subsurface flow. The authors could state why they think that an appearance of water in any of their wells is indicative of a location acting as a source area, while an absence of water indicates an area is not a source. The authors did not look at runoff generation processes, but water table response, so if they want to claim an area sampled by a well was a source of runoff that influenced streamflow response at the bottom of the basin, they need to show it was hydrologically connected to that point in the stream.

- That's a good point. By "runoff source areas", we really mean "shallow rapid runoff response areas". We will revise the text to be more specific about this issue.

In how many of these wells did the water table reach the topographic surface? This is important because the authors need to better understand and explain what type of hydrological connectivity occurred; was it surface or subsurface? Is there a difference? There is neither data nor evidence provided to justify the statement that runoff source areas expand and contract in this watershed.

- Agreed. We didn’t demonstrate hydrologic connectivity; thus, we will reduce our emphasis on this issue. Building upon our point in the previous bullet, we acknowledge that our data demonstrated expansion and contraction of shallow rapid runoff response areas, and that deeper runoff source areas likely existed and may or may not have been spatially dynamic. We will revise the text accordingly.

Figure 5.10 implies that the area contributing groundwater flow to the stream may be variable. Or did the water table merely rise up and down, and the contributing area stay the same? Is what the authors talking about really a "source (or control) volume"? Perhaps the efficiency of the connection improved as the water table rose, but that is a very different thing. The nuance of which I think our community has not fully grasped.

- Good point. We agree that we cannot refute either case based on our data. We will refocus the language to specify that shallow rapid runoff response areas expanded and contracted, and that the runoff efficiency likely varied accordingly.