

Interactive comment on “Controls on groundwater response and runoff source area dynamics in a snowmelt-dominated montane catchment” by R. S. Smith et al.

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In this paper, the authors discuss an investigation to test several hypotheses about controls on groundwater response in a mountainous catchment in British Columbia, Canada. The authors installed wells in a random stratified manner, using metrics of topography, forest cover, vegetation, and soils at each well to determine what catchment characteristics may influence groundwater response. Metrics of groundwater response used were occurrence, and timing and duration of response. 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 ex-

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ample, 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. Either way, the major finding is that topographic controls on groundwater response are not ubiquitous over space and time in this landscape. This is an important finding because it should influence selection of tools for predicting runoff response and streamflow in this type of landscape. 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. 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.

Some minor suggestions could be to 1) Improve the figures that are far too small to read (e.g., 7 and 8). 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? 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. 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. 5) On Page 2575, explain what is meant by “deep disconnected flows”? Disconnected from what, yet presumably flowing to somewhere? 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

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process by which thinner trees increase the duration of groundwater response (Table 4, Figure 5 and Page 2568)? b) How does insolation increase the annual duration of groundwater response? 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. 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. 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.

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