

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

Anonymous Referee #1

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General Comments

Smith et al. have conducted a potentially interesting study on physiographic and meteorological controls on the occurrence, timing and duration of shallow groundwater response in a mountainous catchment with a snowmelt-dominated hydrologic regime in British Columbia, Canada. The goal of their study is to examine the role of spatially variable water inputs (precipitation and snowmelt) relative to topographic controls on groundwater response and runoff source area dynamics. The authors installed a stratified, random network of wells throughout the study catchment and monitored water

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table fluctuations in the wells continuously over one year. Snow depth, snow water equivalent and soil saturation measurements were also taken manually during field campaigns throughout the study period. The authors used ordinal logistic regression modelling to examine the relative importance of a handful of topographic predictor variables (e.g. mean slope gradient, upslope drainage area and soil conductivity), as well as, forest cover, insolation and instrumentation metrics, on the occurrence, duration and timing of groundwater response across the catchment. Different hydrologically-relevant periods and timing metrics were used to examine intra-annual variability of groundwater response. In my opinion, the paper addresses a relevant scientific question that is within the scope of HESS and presents new ideas and good data; however, I find the overall presentation of the material cluttered and hard to follow. I think many of the sections could be reorganized and shortened to improve clarity for the reader. I also have some concerns about whether or not the results are sufficient to support the papers conclusions regarding lateral vs. vertical flow and hydrologic connectivity. In particular, I am concerned that given the deep soils in the catchment, that sites that were deemed unresponsive or transient may not have been installed deep enough to measure the water table elevation. I also am not clear on how the authors are defining hydrologic connectivity in such a large catchment, which is a major theme of the discussion. I recommend that the paper be returned for major revision before it is considered for publication in HESS. I have divided my comments below into specific comments addressing individual scientific issues and technical corrections (spelling, grammar, etc).

Specific Comments

1. Throughout the manuscript, the word ‘persistence’ seems to be used interchangeably with ‘duration’ (e.g. P2550, L13). I recommend just using ‘duration’ since it is presented as one of the response variables in section 2.3.3.
2. In the abstract, the authors state that “Runoff source areas expand and contract throughout these periods according to an interplay between catchment wetness and

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the spatial patterns of topographic convergence.” I find “interplay” too vague a term and suggest the authors be more specific about the interaction between catchment wetness and topographic convergence.

3. On P2550, L16 of the abstract, the word “differential” is used. I think the authors should be more specific about what type of variability they are referring to (spatial, temporal).

4. When discussing the ‘fill and spill’ concept, the authors should reference Spence and Woo (2003) where the term was first used.

5. Are there other studies that can be referenced along with Redding and Devito (2008, 2010) on P2552, L3. While these two papers do discuss the mechanisms involved in lateral and vertical flow in deep glacial soils, they do so by way of an irrigation experiment, which creates rare conditions (in the Boreal Plain) of soil saturation and high intensity precipitation events. Haught and van Meerveld (2011) may be appropriate here or elsewhere in the Introduction, although their soils are relatively thin compared to this study. But, this paper doesn’t really address flow paths deeper than 2m.

6. On P2552, L4 and elsewhere, the authors use the term ‘water inputs’. I think it would be helpful to be more specific here that you are talking mainly about spatial variability in snowmelt inputs.

7. In the last paragraph of the Introduction, the authors lay out their six hypotheses. I think that this paragraph should also include a clear statement of the overall goal and specific objectives of the study. While this can be inferred from the preceding discussion of knowledge gaps in the area of groundwater dynamics in large snowmelt-dominated catchments, I think it is best to be explicit about what is in the current study.

8. Be specific about what the ‘other factors’ are in your first hypothesis statement.

9. I would like to see some foundational discussion in the Introduction to support the third hypothesis statement regarding intra-annual variability of groundwater response.

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It sort of pops up out of nowhere.

10. The fourth hypothesis statement is too wordy and hard to follow.

11. The term ‘groundwater response’ should be defined earlier in the Introduction (probably in the second last paragraph on P2553). Does this mean that the water table is measured in a well?

12. I think there are too many hypotheses being tested here. Can some of them be combined? Hypothesis 3 seems redundant.

13. In section 2.1, where the authors give an estimated range of ET, I would like more information on how these values were modelled. If they don’t want to take up space describing it here, the information could be moved to the Supplementary Material.

14. On P2555, L21 the authors state that spring snowmelt dominates the hydrologic regime. It would be useful to quantify this somehow, e.g. what percentage of annual flow is from snowmelt?

15. P2555, L23-25: It would be helpful to know how many years this range covers.

16. P2556, L13-15: I do not understand what is meant by “. . . based on the USDA soil classification system (Smith, 2011).” No classification information is given here, just data on soil texture and structure. If some sort of classification is intended, why isn’t the Canadian System of Soil Classification used?

17. P2556, L15-19: Where were observations (or lack thereof) of soil macropores or cracks made? Soil pits? Road cuts? This should be stated. Also, is it really possible to visually assess the abundance of burrowing animals and insects over such a large catchment?

18. P2557, L22: Was more than one well installed at each site?

19. P2557, L25: Does ‘groundwater initiation’ mean the water table moves up into the well and/or an existing water table elevation changes? Please clarify.

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20. I think that the authors need to provide a bit more information on why wells were installed in soil pits that were then backfilled. Depending on the depth of the pits there could be significant disturbance to the soil structure surrounding the well, no?
21. P2558, L9-11: Specify what water table depth was measured relative to. I assume it is the ground surface or a benchmark, but be specific.
22. P2558, L19-22: Be specific about what depths soil saturation was measured at.
23. P2558, L26,27: I think that all methodology should be outlined in this paper or its Supplementary Material. Realistically, not many people are going to want to dig up a PhD thesis to find more info.
24. I don't think Table 1 is necessary. It repeats a lot of the information already given in the Methods section and anything extra could be easily added to the text or Supp. Mat. The authors may want to consider using italicized subtitles to break up section 2.2 into more digestible and easy-to-find subsections.
25. P2559, L3: Reference Figure 1 after "... lysimeter sites..."
26. P2559, L4-6: See specific comment 23.
27. P2559, L7-14: Some mention of how these other sites compare to the study area should be made, especially for the SWE data which was obtained from a site 12km away.
28. P2559, L17: What temperature were soils burned at and for how long?
29. P2560, L6-8: Could the Ks calculation be described briefly here or in the Supp. Mat.?
30. Overall, I find section 2.3.3 on Statistical Analyses very cluttered and confusing. I think this section could be broken down into subsections (e.g. 2.3.3.1 Groundwater Response Classifications, 2.3.3.2 Data Transformations, 2.3.3.3 OLR). Also, a lot of the background theory on OLR could be moved to the Supp. Mat.

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31. On P2561, L25-27, groundwater dynamics are classified into three classes, persistent, transient and unresponsive. Then in the following sentence, the authors use different terminology, e.g. 'temporally discontinuous' and 'detectible'. Why not be consistent with the terminology? I also don't quite understand where this classification comes into play. Is it just used descriptively or is it somehow related to the 'Occurrence' response variable. Please clarify this in the text.
32. P2562, L2: "... data censoring..." doesn't seem like the right term here. If the authors are talking about 'occurrence' here then even no data is meaningful since it is given a dummy value of 0.
33. P2562, L3: "... that did not experience groundwater responses..." Why not use the original terminology that you laid out, i.e. 'unresponsive'?
34. Building further on comments 11, 19 and 21, I am starting to wonder if any of the unresponsive wells were actually just not installed deep enough to measure the water table depth relative to the surface. I would like to know what the depths of the wells at the 13 unresponsive sites were. I fully understand the difficulties in installing wells into hard and/or cobbly soils, but since the whole point of this study was to look at groundwater dynamics in deep soils, this possibility should be addressed in some way.
35. Are only the parameters with a symbol in Table 2 used in the OLR? If so, this needs to be specified in the caption and text.
36. It seems like the paragraph on the three types of response variables (P2563, L8 to P2564, L2) should be moved to before the description of OLR. This sets the stage for exactly what is being investigated and needs to be set out before the OLR discussion and the descriptions of the eight distinct hydrological periods.
37. Since all the wells were installed to different depths, shouldn't 'duration' be computed as the fractional portion of time that a water table was recorded at a specific depth below the ground surface and not just the duration of time it was in the well? And

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just because the water table drops below the bottom of a well, it doesn't mean that it's not there. Again, I find this measurement confusing. This comment also applies to the 'timing' response class.

38. P2563, L17-20: What do 'transient perched' and 'continuously persistent' mean? Again, can the original terms that were set out (persistent, transient, unresponsive) just be used here? Also, why were they treated as one population?

39. Table 3 and the description of the different response variables and classes MUST be revised for clarity. As is, it is very confusing. Table 3 should include a new column on the far left titled 'Groundwater Response Class'. In this column, 'Occurrence', 'Duration' and 'Timing' can be listed. Leave the 'Period/Timing' column as is, but specify that occurrence and timing use the full annual dataset (I assume that is right?). I don't understand what 'Class 0', 'Class 1' and 'Class 2' listed under the 'Range of Responses' heading mean. Also, since this table is showing the range of responses for each class, this should be at the start of the caption. . . i.e. "Range of responses for occurrence, duration and timing response classes. . ." then go on to describe units.

40. P2564, L7: ". . . some circumstances. . ." is vague. Be more specific.

41. P2565, L17-19: Does it make sense to include well depth as a predictor variable? It seems very likely that well depth would affect whether or not a well was responsive or not.

42. The OLR methods on P2564 are long. Could some of this be written more succinctly, combined with the previous OLR theory, or moved to the Supp. Mat.?

43. Figure 3 is very difficult to interpret. The grey scale used for potential radiation and snow cover extent is difficult to differentiate. Perhaps colour would work better in this regard. Also, is it necessary to keep the harvested area marked? It clutters the potential radiation map.

44. Maybe the description of snowline retreat in section 3.1 could be represented in

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Figure 3b instead of snow extent for each survey, which is difficult to interpret.

45. P2566, L12-17: The authors state that the location of unresponsive wells was consistent with the model results, which is fine since those wells were not included in the model. However, I don't understand what is meant by "The spatial distribution of deep soil Ks was also generally consistent with the model results based on a manual comparison." Was Ks being modelled? Please clarify.

46. It is very hard to see the two light grey lines in Figure 4.

47. Again, 'persistence' is used in lieu of 'duration' in the caption for Figure 4. Be consistent.

48. The left-hand column of Table 4 should be revised in the same way as I have suggested for Table 3 in comment 39.

49. The organization of the Results section is not intuitive to me. Why not organize it according to the response classes (occurrence, duration, timing)? Instead, occurrence and duration for the melt and annual periods are discussed in one section, timing in the next, and then duration for individual hydrologic periods. I find the current structure of the section detracts from the interesting results.

50. There is a lot of detail in section 3.4 that I think can be distilled down into the most important observations.

51. It is very hard to distinguish adjacent circle diameters in Figure 6 and I can barely see the 'no response' sites. While I like the idea of mapping the data, I'm not sure that this particular figure is the right way to do it. Maybe if annual and melt periods were displayed on separate maps the figures would seem less cluttered. This figure deserves more thought.

52. Figure 8 is MUCH too small and some of the lines are far too faint. On P2569, L22-24, the authors state that we can see the relationships shift to higher or lower values of the predictor variables; however, this is difficult to pick out. Figure 10 does clarify the

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observation; however, I would use the full names of each period on the x-axis instead of the numbers here. This makes it easier for the reader to interpret the changes without having to refer back to the period descriptions in the methods section.

53. P2571, L23-26: Is deep soil Ks the most important for all periods?

54. P2572, L4-8: The authors compare their results to those of Redding and Devitio (2008, 2010); however, they do not compare Ks to the intensity of water inputs as Redding and Devito do. The statement that their results are consistent with the percolation-excess runoff generation mechanism seems like a jump and I think requires more evidence.

55. P2572, L18-21: I think there needs to be more discussion of the snowmelt and insolation patterns (Figure 3) and how they relate to spatial patterns of groundwater response to substantiate the statement re: controls on snowmelt intensity and hence groundwater response.

56. P2572, L25-28: A mechanistic explanation of the relationship between tree diameter and vertical vs lateral flux partitioning is needed here.

57. P2573, L2: How do you know that most of the catchment is “hydrologically connected”? Based on Figure 5.10 in the Supp. Mat., there are quite a few unresponsive sites between the responsive ones during the very wet snowmelt period. I think that you can only infer that the near-stream sites are connected, especially since the wells are sometimes hundreds of meters apart. Along these lines, I think you need to define what conditions infer hydrologic connectivity in this particular catchment, preferably in the Methods section. Some examination of the relationship between groundwater fluctuations and discharge, as well as the relationship between groundwater fluctuations in adjacent wells might be helpful in this regard.

58. P2573, L15 to P2573, L7: This paragraph is very wordy and hard to follow. I think it could be shortened and written more succinctly.

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59. P2574, L8-11: There needs to be more of a mechanistic explanation given here.

60. In section 4.4, a lot of attention is paid to the results from other studies where I think the focus should be on the implications of the current study.

61. Also in section 4.4, I think there needs to be some discussion of how the models presented in Table 4 can be tested. Along these lines, it would have been nice to see the models verified in some way, for example, by testing on an adjacent area with similar characteristics.

62. Some of the wording in the Abstract is repeated verbatim in the Conclusions. For example, the sentence “Runoff source areas expand and contract... spatial patterns of topographic convergence;” is repeated verbatim.

Technical Corrections

P2550, L10: “. . .distribution of sites. . .” is vague. Perhaps change to “. . . spatial distribution of sites. . .” or “. . . number of sites. . .”

P2560, L10: Change “Analysis” to “Analyses”

P2561, L19: Same as last correction.

P2561, L21-24: The sentence “Notwithstanding the fact. . . in the UEC catchment.” is long and awkward. It could be reworded more simply as follows “Although we do not have data for a full year, the missing data from late September and October 2008 are not of great concern since this is a relatively dry period in the UEC catchment.” Or something like that.

P2567, L23 and P2567, L6: Change “advanced” to “earlier”.

P2567, L25 and P2567, L7: Change “delayed” to “later”.

P2572, L22: Change “persistence” to “duration”.

P2574, L18: The word “initial” seems unnecessary here.

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P2576, L7: "differentiates" doesn't seem to fit here.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 2549, 2013.

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