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Hydrology and Earth System Sciences Discussions

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 #2

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The authors present a creative statistical analysis of a valuable dataset on the spatial and temporal persistence of groundwater dynamics in a snowmelt dominated catchment. The dataset is certainly worthy of publication. The paper is well written, but there are some issues that I feel need to be resolved before it is ready for publication. Below, I present some general comments follow by page,line specific comments.

General Comments. 1. I think the conclusions and implications presented stray a bit too far from the data and results. This could be rectified by clarifying and justifying some assumptions. a. A dominant theme in the introduction and discussion (although

not so much in the results) is the impact of spatially heterogeneous melt on runoff generation. However, there are no melt rates presented. Rather, we see a map of melt timing along with insolation calculations. A key assumption is that melt rate is directly correlated with insolation. Are instantaneous melt rates linked to the timing of melt areas? Methods state that melt rates were measured in lysimeters. Why are the data not presented? b. Everything from the title through to conclusions says that the paper is about runoff generation. Yet, the data are all about groundwater dynamics. There is an implicit assumption that the spatial and temporal distribution of groundwater are linked to runoff generation. This needs to be clarified.

2. Because insolation is so important to the study, we need more information about how it was calculated. Was forest cover take into account? Was it calculated daily? What metric of insolation was used in the OLR?

3. It seems that the authors really wanted to criticize topography-based hydrologic models and so looked for instances in their data to support that idea. I think actually their data show that for most of the time such models will indeed work fine. For example, lines 12-16 in the abstract quoted below pretty much support the idea that topography based models WILL work during the most important runoff periods. Yet they highlight the period when they might not. "Upslope contributing area and slope gradient are first-order controls on the persistence of groundwater response during peak flow, recession flow, and low flow periods. Runoff source areas expand and contract throughout these periods according to an interplay between catchment wetness and the spatial patterns of topographic convergence."

4. The statistical analysis in the methods comes as a surprise. The introduction should briefly summarize how the methods are used to address the goals.

5. The general goal of the paper is a bit hidden in the extensive list of detailed hypotheses presented in the introduction. I suggest writing some general goals and objectives, and then rephrasing the hypotheses to conclusions. They really are better suited as

conclusions.

Specific Comments 2550, 20: I think this sentence is misleading. Topography-models generally use upslope contributing area and slope as the primary controls. Both of these variables are strong predictors of groundwater occurrence in this study. Yet the authors choose to highlight the times of the year when such models MIGHT fail, rather when they will likely succeed.

2551, 11: I don't think non-technical colloquialisms such as "fill and spill" belong in technical papers. They are fine in conference talks when the point is to tell an entertaining story and engage the audience. More informative and technically precise terms should be used in papers. I understand that this particular phrase has become popular in the watershed hydrology community, but it is misleading. Perhaps storage excess would be better.

2553, 28: While it is true that few studies have addressed groundwater dynamics in response to asynchronous water inputs, the finite list implies that these are the ONLY studies to have done so. I suggest adding an e.g. to the reference list. Other studies that could be cited include

Hinckley et al (2012) Aspect control of water movement on hillslopes near the rain-son transition of the Colorado Front Range. Hydrological Processes, DOI.1002/hyp.9549.

Smith et al (2011) Small storage capacity limits benefit of winter snowpack to upland vegetation. Hydrological Processs, doi: 10.1002/hyp.8340.

2554, 9-27: The hypotheses read more like conclusions. I think this is too much detail for this point in the paper. If they are indeed hypotheses, then each one should be explicitly addressed in the results and discussion. If they are conclusions (I suspect they are because none of them are refuted), they should be generalized in the introduction, or posed as problems statements. Many of the hypotheses have not been introduced yet, so the reader has no context. I suggest rewriting this section to say

something like: This paper investigates the relative importance of topographic, biotic and energetic controls on groundwater dynamics. It is hypothesized that the relative importance of these controls vary as the hydrologic seasons progress... Something general like above sets up the problem and gives general conclusions with the detailed list of conclusions. The current list of six hypotheses can then be moved to Conclusions. I would then suggest adding a statement to say that the above problems were addressed by relating the temporal persistence of groundwater dynamics to landscape properties using OLR. Such a statement would better set up the methods.

2557, 3: How was insolation calculated? Was forest cover take into consideration?

2559,1: Soil moisture data are never presented. No need to introduce.

2560, 12: The first sentence can be deleted. " Begin section with A 5m DEM..."

2561, 1: Why are there no snow parameters?

2561, 21: The purpose of the statistical analysis has not been established. I suggest rewriting the introduction to set up this method.

2562, 18-20: Some statement like this should be in the introduction.

2565, 11: "2007 and 2008" implies 2 separate winters. I suggest "the winter of 2007-2008"

2565, 9-19: This section is not well connected to the rest of the study. The paper introduces snowmelt variability as one of the main points of investigation, yet this limited analysis is the only places snow appears in the results.

2574, 1-4: We really don't know anything about snowmelt rates. Figure 6b tells about snowmelt timing, but that is very different than snowmelt rate. Where are the lysimeter data?

2574,10: There is no hypotheses about forest cover removal on page 2554.

2576, 7-18: This discussion should be more prominent to align the paper with the stated goals.

2577, 25: Topography-models generally use upslope contributing area and slope as the primary controls. Both of these variables are strong predictors of groundwater occurrence in this study, yet the last line of the conclusion says that such models will be poor predictors of runoff dynamics during the early phases the spring freshet. I think this is overstated, and it ignores the results that such methods will work well during most of the freshet.

Table 3: Class should be defined in the caption

Figure 5: This is very difficult to understand. The terms “effect size” or “effect size class” never show up in the text. The reader has to figure out the plot from the captions, which are generally quite informative, but I spent too much time trying to understand how this plot illustrated the strengths of variable effects. It’s still fuzzy, and I never did figure out what the numbers next to the symbols are for.

Table 2: There are no snow variables.

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