Author response to reviewer #2 comments for HESS manuscript "Controls on groundwater response and runoff source area dynamics in a snowmelt-dominated montane catchment" [Paper #: hessd-10-2549-2013]

Dear Sir or Madam,

We would like to thank you for your detailed and thoughtful review of the original manuscript submission. You raised several important issues that will certainly result in a stronger manuscript. 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

Reviewer #2 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?

- Those are good points. We will reference findings from two published manuscripts (Jost et al. 2007, Jost et al. 2012) that address the topic in detail for the watershed. The first showed that insolation, elevation, and forest cover were the main predictors of melt rates in the catchment. The second incorporated some of the lysimeter data in modelling snowpack processes.
- Jost, G., Weiler, M., Gluns, D.R., Alila, Y. 2007. The influence of forest and topography on snow accumulation and melt at the watershed-scale. Journal of Hydrology, 347, 101–115.
- Jost, G., Moore, R.D., Smith, R., Gluns, D.R. 2012. Distributed temperature-index snowmelt modelling for forested catchments. Journal of Hydrology, 420–421, 87–101.

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.

- Good point. That is an assumption being made. The second paragraph in the introduction was intended to address this issue. We will revise the introduction to address this connection more directly. We will also change the title in light of this issue (e.g. "Spatial controls on groundwater response in a snowmelt-dominated montane catchment").

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?

- Several of these details were provided in section 2.3.2. In addition, we will specify that forest cover effects were excluded from the modelling.

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."

- We were not intending to criticize topography-based models. Rather, we intended to identify that other factors can be important in snowmelt-dominated catchments and that topographic factors can't necessarily be relied upon for modelling all phases of the hydrograph. We will revise the text to make sure our intentions are clear.

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.

- We agree. We will revise the last paragraph in the introduction to state the study objectives and briefly explain how we address them.

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.

- We agree. We will revise the text accordingly.

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.

- We believe we took a balanced approach to highlighting when topography-based models should perform well and when they might be problematic. Slope gradient and upslope area were the most important parameters for predicting groundwater response duration during most periods; however, 75 cm Ks and/or maximum tree diameter were more important during early phases of the spring freshet, and 75 cm Ks was the most important parameter for predicting groundwater response occurrence. These findings could be quite important depending on one's purpose for modelling runoff (e.g. a person trying to predict ice breakup on rivers driven by early freshet flows would care more about early spring runoff and less about peak flows). However, we will revise the text to clarify that we're not specifically intending to criticize topography-based models.

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.

- We will adopt the term "storage excess", but will also reference "fill and spill" upon the first use since "fill and spill" is in common use. We will also reference Spence and Woo (2003), since they were the first to use the term.

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.

- Good point. We will make these modifications. Thank you for the references.

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.

- We agree. We'll revise the text accordingly.

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

- Please see response to comment 2 under general comments.

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

- Soil moisture data are presented in section 4.2 and Figure 9.

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

- Good point. We will delete the sentence.

2561, 1: Why are there no snow parameters?

- We didn't include snow parameters because we chose to analyze stationary (or relatively stationary) physiographic variables. Snow condition would be captured implicitly through its influence on groundwater response. Moreover, including parameters describing snowpack condition would weaken the apparent effects of the other parameters in the model because snowpack is influenced by the other parameters (i.e. it's a co-variate). We believe it would be more appropriate to treat snowpack as a response variable and apply a similar approach as that followed in this study. An example of this approach is provided in Jost *et al*. 2007.

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

- We agree. We will revise accordingly.

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

- We will specify in the introduction that we studied variation in groundwater response for hydrologically distinct periods of the annual hydrograph.

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

- The snow cover patterns shown in Figure 3b were for two separate winters. 2008 was the year analyzed in the article. However, the frequency of snow cover surveys was low, so the snow cover patterns for 2007 were also presented to show the general patterns of snowline retreat. The text will be revised to clarify this point.

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.

- Please see response to comment 1a.

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?

- Please see response to comment 1a

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

- Agreed. We will revise the introduction as stated in our response to comment 4 of the general comments.

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

- We will reorganize the discussion to address this concern.

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.

- We will revise the text to indicate that topographic factors would not likely be good predictors during early phases of the spring freshet, but would likely increase in importance as the freshet and post-freshet periods proceed.

Table 3: Class should be defined in the caption

- Agreed. We will clarify the table and caption, as discussed in other responses above.

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.

- Good point. “Size” will be removed from the terms mentioned, and we will explain the calculation of the effects in section 2.3.3. We will also revise the explanation, as follows:

- "The effect of each predictor variable was calculated as the exponential of the following product: the coefficient for the variable multiplied by the range in the data between the 25th and 75th percentile values for the variable. Interaction terms were ignored for calculating main effects. For calculating interaction effects, the corresponding interaction variables were held at their respective minimum or maximum values."

Table 2: There are no snow variables.

- Please see corresponding response above.