Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-98-RC1, 2019

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Partitioning snowmelt and rainfall in the critical zone: effects of climate type and soil properties" *by* John C. Hammond et al.

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Received and published: 27 March 2019

Preliminary comment Bettina Schaefli has been invited by the editor to review this paper. She discussed the paper extensively with her PhD student Harsh Beria and they wrote the review together.

Summary and general recommendation This paper adds to the rapidly increasing body of literature that tries to understand the differential impact of rainfall versus snowfall on streamflow generation and groundwater recharge. It compares the effectiveness of rainfall and snowmelt in streamflow generation and deep groundwater recharge using a 1-dimensional physically based subsurface model (Hydrus-1D).

This study is a pure modelling study (virtual reality-type analysis) but it uses observed snow water equivalent data (SWE data) to make the model experiments realistic in different climatic settings. The analysis is very comprehensive and to our knowledge, the first study that uses a physical subsurface modelling approach in this context. The investigation is carried out in three mountainous regions in the US with their climate varying from humid to semiarid. The effects of soil depth, soil texture, rainfall and snowmelt input rate, and catchment antecedent moisture conditions are investigated. The main conclusion is that surface runoff will decrease with lower amount of snow in the future. The effect on groundwater recharge is not very clear from the presented results. Groundwater recharge is estimated to be higher with increasing liquid rain in wet regions of the western US. The effect in dry regions is more dependent on soil depth and texture.

Based on the above, the paper is highly suitable for publication in HESS. Some detailed suggestions for further improvement of the manuscript are given below.

Major comments

1. The authors find that, overall, high intensity melt events are more effective in producing groundwater recharge in dry regions. This is in line with previous investigations (Jasechko Taylor, 2015) and could be stated more clearly in the abstract and conclusions.

2. The abstract is not very concise since it is not entirely clear from the abstract alone what was unknown beforehand and what the study confirms.. Currently, the abstract seems more like a list of everything that was done in the study and it is hard to get the key take away points. The authors might want to reduce the abstract length and condense the key take away points..

3. The paper mostly shows how streamflow generation and groundwater recharge depends on snow fraction. As has been shown in past (Barnhart et al., 2016; Musselman et al., 2017), snowmelt rates (i.e. melt intensity) and not snow fraction control hydro-

logic partitioning. The study would greatly benefit from reporting additional insights about h ow streamflow generation and groundwater recharge depend on snowmelt rates in the different climate regimes, and from a comparsion of the model results with previous observation-based studies.

4. The authors do not consider rain-on-snow and mixed precipitation events in this analysis. This is probably because they work with SWE data obtained directly from the SNOTEL network, and do not use a snow model. It might be useful to mention the different event statistics in the text (eg: how much of the annual precipitation is in the form of mixed events). This should also be mentioned as a limitation in section 4.2 (which would probably benefit from a change in the title, “limitations” instead of “uncertainties”).

Detailed comments

P1 L24: Snowmelt fraction is not a commonly used term. Can instead write snowmelt as a fraction of annual precipitation. Alternatively, snowmelt fraction can be defined a priori and then be used.

P2 L57-58: The phrase “rainier futures” is a bit awkward. Remove?

P2 L74: “energy hinders.. “. Do you mean that solar radiation is low during early melt and not supportive enough to drive vegetation growth, assuming snow melts very early? Then how will the growing season length increase? Should be clarified.

P3, line 82: “moisture content on north-facing slopes”: moisture content of the snow pack, of the soil? This requires a reference

Variable names should be in italics (examples: L101-102, L138-145). The usage of semicolon (;) in lines 101 and 102 is incorrect and reading these sentences is difficult. Please check the usage of the semicolon throughout the paper.

P3 L105: The water balance equation is written in terms of volumes rather than fluxes, which is unusual; a change to fluxes (with the change in storage over a time increment,

deltaS/deltat) seems more appropriate.

P3 L101 vs P4 L140: In L101, runoff is defined as lateral export of water from the domain. In L140, runoff is defined as overland flow resulting from saturation- or infiltration-excess . Please clarify how runoff is defined and make it consistent throughout the manuscript.

P6 L221: Mention what percentage of annual precipitation is accounted by mixed events

P7 L230: for the calendar year or the hydrological year? If the calendar year: how is the snow carryover handeled?

P7 L242-244: Define PCI and ICI indices with the possible range and mentioning what increasing PCI/ICI values mean

P7 L253 and following: it is not clear what was tested via indicator-variable regression; what is indicator-variable regression? Which results are based on this?

P9 L327: Not clear based on the second panel of Figure 8a that you can arrive at this conclusion.

P10 L371: Also mention Figure S2 which shows that a transition from snow-dominated regime to a rain-dominated regime may increase the amount of deep drainage. The Figure S2 is probably worth including in the main paper.

P11 L388 – 390: Which figure is this argument based on? If it is based on Table 3, please reference it in the text.

P11 L402: How do the relative amounts of Q and D change? It is more useful to state how they change than simply mentioning they change.

P11 L406: Incorrect figure citation.

P13 L458: Figure S2 does not show difference between model runs using daily vs hourly inputs. Incorrect figure citation.

Usage of the phrase “input concentration” is a bit ambiguous (eg: L203, L376). The authors use concentration in terms of length of melt period or temporal clustering of events. However, it can also be misunderstood as the intensity of melt. This should to be clarified in the text.

There are a number of very long sentences in the paper which can be reframed to make the text easier to read. Some examplesare: L81-85: This can be broken into two sentences. L116-120: The sentence can be shortened or converted into two sentences. L438, 464: The use of phrase “biased wet“ does not sound right. L466: Reframe the sentence

Figure specific comments:

Figure 2: What does different shades of gray mean? Is it related to hydraulic conductivity?

Figure 3: In the second panel of (B), the y-axis label does not correspond with the figure label description. In the y-axis label, S/P is written whereas in figure label, ΔS/P is written.. What can we learn from panel B? Would it be interesting to present a Budyko-plot instead?

Figure 5: Put correlation values in the figure

Figure 6: * means P-value of <0.5. Did the authors mean to write < 0.05? Its uncommon to report p-value of < 0.5. The same p-values have been reported in Tables 3, S3, S4 and S5.

Figure 8: Instead of plots of Q and D, might want to show cumulative plots of Q and D as not much can be clearly seen in Q and D plots. Increase contrast between lines corresponding to 1.5x and 2x of soil depth.

References: Barnhart, T. B., Molotch, N. P., Livneh, B., Harpold, A. A., Knowles, J.

F., Schneider, D. (2016). Snowmelt rate dictates streamflow. Geophysical Research Letters, 43(15), 8006–8016. https://doi.org/10.1002/2016GL069690

Jasechko, S., Taylor, R. G. (2015). Intensive rainfall recharges tropical groundwaters. Environmental Research Letters, 10(12), 124015. https://doi.org/10.1088/1748-9326/10/12/124015

Musselman, K. N., Clark, M. P., Liu, C., Ikeda, K., Rasmussen, R. (2017). Slower snowmelt in a warmer world. Nature Clim. Change, 7(3), 214–219. https://doi.org/10.1038/nclimate3225

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