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Hydrology and Earth System Sciences
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.

Anonymous Referee #2

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Overview: The manuscript submitted by Hammond et al., applies a 1D variably saturated subsurface flow model to three different regions in the western United States to explore how rain or snowmelt is partitioned in the subsurface as a function of climate (humid to semi-arid) and soil conditions. Numerical experiments are run to test the sensitivity of hydrologic partitioning of ET, runoff (Q) and deep drainage to P type and intensity and soil characteristics. Results indicate drier climates are more sensitive to snowmelt runoff generation and deep drainage compared to wet climates. Seasonal sensitivity of Q to soil texture does not scale to annual variability across soil types tested, while soil depth is particularly important to partitioning if soil storage is less than annual mean precipitation. Deeper soil profiles are found to produce more ET,

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less runoff and more deep drainage

General Comments: The submitted manuscript is a contribution to new ideas in the hydrologic sciences. Understanding the partitioning of soil water, runoff and deeper percolation below the root zone are important to understanding weathering, plant water use, stream water source and travel time distributions. I really enjoyed this paper. It is generally well written and well cited. I recommend the paper for acceptance with minor revisions. Specifically, I have no recommendation for additional modeling efforts. My main criticism are (1) I feel the key points are not well defined for the reader, and (2) the "Discussion Section" a bit repetitive of the results. Instead, the paper would benefit from more insight (compare and contrast) of this work with previous literature results and thoughtful hypotheses for how more complex boundary conditions may influence results (i.e. future work). Instead, I feel the reader gets bogged down in detailed results (that are in the Results section) and the key points are sort of lost.

Specific Comments:

(Introduction) I very much enjoyed the Introduction. It is well written. I especially enjoyed your section on soil conductance, diverging patterns in growing season length. Consider looking at and adding Knowles et al., 2018 GRL paper (doi.org/10.1002/2017GL0706504) to the Intro. The introduction should provide the reader with some hint at the limiting assumptions of a 1D approach, as I found it took too long to mention complex topography and lateral flow in my initial read through. Only in the Discussion Section "Uncertainties" is this brought to the attention of the reader with a fairly nice review. I recommend bringing some of this literature review to the Introduction. In addition to rooting depth, I suggest some discussion of how above-ground vegetation influences snow accumulation and melt rate.

(Methods) Page 5, line 151: Did you explore sensitivity of PET derived from a coarse grid of 4Km, as this is likely not representative of a single SNOTEL site given mountainous terrain.

Page 6, line 190: I am ok with the conceptual model but I think one needs to consider the implications of removing the lower boundary effects on the solution through free drainage in the discussion. Specifically, I am thinking of Brantley et al., 2017 paper where she nicely states in the abstract, "water can also flow laterally in the shallow subsurface as interflow in zones of permeability contrasts. Interflow can also be perched or it can occur during periods of high regional water table".

(Discussion) Page 11, line 392, "once subsurface storage is at capacity, D will plateau and Q will increase with further input due to the saturation excess mechanism". This is a very interesting conclusion, can you provide more evidence through previous research that this result is a physical representation and not a result of the model construct.

Page 11, line 409, interesting result that soil water storage<mean annual precipitation, and you do provide the Smith et al., 2011 reference. But I would like to see more literature on the soil storage capacity, D and Q relationship; perhaps bring in how this might influence where D is generated (or not generated) across the watershed.

Page 12, line 416. Consider renaming this section from Uncertainties to something like "Limiting Assumptions" as you do not actually address uncertainty mathematically. While this section is fairly complete, consider speculating on how your results may be different by including (a) transient LAI (look at Kim et al., 2018, GRL doi.org/10.1029/2018JG00438) for some discussion ideas on phenological response to warming induced earlier green-up, (b) potential lower boundary condition controls imposing on the solution – i.e groundwater, (c) complex topography of slope and aspect and (d) lateral flow.

(Conclusions) Page 14, line 485. Your last sentence is not very strong, "water managers should develop strategies to mitigate ....". It should contain some qualifications based directly on your analysis.

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