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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Importance of maximum snow accumulation for summer low flows in humid catchments" *by* M. Jenicek et al.

Anonymous Referee #2

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I reviewed the paper "Importance of maximum snow accumulation for summer low flow in humid catchments" by Jenicek et al. Overall, I am quite intrigued by this topic, hence my reason for reviewing the paper. However, I found the paper lacking a clear take-home message and was often confused by the writing and organization of the paper. The authors lay out two interesting research questions: 1) determine the length of memory effect on low flow conditions for maximum SWE and 2) estimate the sensitivity of catchments to changes in snowpack. While I find these questions compelling they don't seem to be very well answered by the study (see major comments below). In particular, the authors seem to neglect discussing the very high correlation between low flows and winter precipitation (almost always explaining more variability than maximum SWE). I think this may be an opportunity, rather than a limitation, to identify a

novel response (see major comments). Shifting the study questions to better reflect the [potential] novelty of the work is needed. Secondly, I do not feel that the authors adequately address their second question about sensitivity to changing snowpacks. They seem to suggest that high elevation catchments are as much or more sensitive to warming as low elevation catchments. This seems like a large simplification (see major comments below). I lay out several potential ways to reframe the work that may help address its novelty.

Major comments:

Novelty of the work: To me the important questions for a climate like Switzerland are 1) does changes in the timing of snowmelt or changes from winter snow to rain alter summer baseflow? and 2) can summer precipitation counteract the effects of changing winter precipitation inputs? The paper currently feels like it is arguing that SWE is more important than precipitation (which is not supported by the results) and that SWE becomes more important during dry summers (which is really not surprising). From my perspective the real question is does SWE (timing or amount or S/P) explain additional variability in low flows beyond what winter precipitation explains. This may require a different analysis, possibly normalizing for winter precipitation or some type of step-wise regression. One might hypothesize that snowpacks release water later in the year, so the timing of snow disappearance may be the critical information (in addition to winter precip amount). Currently, the paper suffers greatly by not discussing that winter precipitation explains as much or more variance of low flows than SWE variables (Table 3). I also suggest that the authors use wet and dry summers to ask when summer precipitation can overcome poor snowpacks or dry winters. Perhaps this could be accomplished using an elasticity type relationship for both summer and winter precipitation and SWE. Do you need more summer precipitation to drive the same low flows that winter precipitation (i.e. winter precipitation is more efficiently partitioned to streamflow)? This is an important question that has large climate change impacts. Along those same lines, I strongly encourage the authors to move away from

their second research question about sensitivity to changing snowpacks unless they significantly bolster related analyses (see comment below). This is a great discussion point, but currently poorly addressed.

Climate change effects are oversimplified: The authors use elevation as a means to organize the catchments and their sensitivity to SWE. This seems problematic given that there is generally a large gradient of precipitation and S/P ratios across elevation. The discussion seems to imply that high elevation catchments are as much or more sensitive as low elevation catchments. This may be true if catchments are all near zero degrees and precipitation is evenly distributed across the winter, however, this is not discussed. I can imagine situations where high elevation catchments are less sensitive to a given amount of warming because they are well below 0 C for most of the winter. Given that the authors do not partition variance well between winter precipitation and SWE effects on low flows, I think the discussion of climate change is very weak. It is quite possible that the points suggested above may improve discussion points here, however, I suggest the authors do not make that a central research question but a discussion point (or significantly bolster the associated analyses).

Use of monthly/weekly low flows: I am mixed about the use of monthly/weekly low flows. On one hand, this fits with the question about memory effects on low flows that the authors pose. It also gets around potential issues with noisy annual low flow data. On the other hand, what is a low flow in May and why does anyone care? I find the use of the lowest summer flow as a much more compelling response variable to predict. The use of monthly low flows is particularly problematic early in the summer when some watersheds are storing water as snow and others are not. In some ways the current effort is quantifying the recession relationship of the watersheds, which is [in my opinion] not the focus of the paper. Perhaps I am missing something here that could be better explained in the text.

Minor comments:

- Abstract has no quantitative results

- Introduction seems to wander from idea to idea without a clear structure. Too many paragraphs that talk about similar ideas.

- I would like to see a table of the mean and CV of all predictor variables and response variables.

- The figures are extremely hard to read in black and white, which many people will do when printed. Particularly Figure 4, 7, and 8.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 7023, 2015.