Hydrology and
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Discussions

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Interactive comment on "Topography significantly influencing low flows in snow-dominated watersheds" *by* Qiang Li et al.

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The manuscript "Topography significantly influencing low flows in snow-dominated watersheds" from Li et al, submitted to Hydrology and Earth System Sciences, attempts to assess the role of topography on various flow variables to identify important topographic indices (TIs). This was done by first defining a set of TIs of which redundant ones were excluded based on a factor analysis. The remaining TIs were used to estimate their relative contribution to the flow variables: each variable separately for each year. This contribution was estimated with linear regression models. This analysis indicated that the contribution of the TIs were greater for the low flows; the most significant TIs were: perimeter, surface area, openness, terrain characterization index and slope length factor. Hence, the topography significantly influences the low flows for this study

region.

The topic addressed in this manuscript is very interesting. In general, the paper is well written: there is a clear structure, complete and concise summary and a clear title indicating the conclusion of this paper. The methods are generally explained clearly with just a few missing details needed for reproducibility, for example on occurrence/contribution of TIs and on the linear regression model. Based on the results shown, it is clear how the conclusions are taken. However it is not possible to verify everything, for example the selection of non-redundant TIs. There are just some drawbacks that should be improved.

General comments:

1) The focus seems to be on having an extensive list of TIs rather than flow variables while both is necessary to thoroughly assess the influence of topography on the flow. Additional flow variables that might be interesting to include are, for example slope of flow duration curve, rising limb density, auto-correlation function or the timing (start of a season, duration of a season).

2) Good idea to exclude redundant signatures, but as a result some signatures are only included indirectly (line 260-262) making it rather difficult to assess their significance on the flow variables. Also, what if a secondary TI selected for the analyses (e.g. DDG), turns out to be significant only because of the primary TI (e.g. slope) included in this secondary TI? So how conclusive are the results with the selection of these TIs?

3) Explain the methodology of the stepwise linear regression model detailed: include whether a fixed order for including additional TIs was used and whether this order is of significance. In the linear regression, TIs were included even though their resulting estimates are very low. Aren't these TIs insignificant and shouldn't they therefore be excluded?

4) Suggested addition to the discussion: under what conditions are the results transfer-

able to other watersheds? For example are results expected to be differently for even larger watersheds (e.g. 150 000 km2) with similar climatic conditions?

Specific comments:

1) Line 42: The term "somehow" is not nice in a paper, it just be removed here

2) Line 94: include an evaluation with ground measurements if possible

3) Line 119: unclear unit of the flow variables: mm/year or mm/d?

4) Line 129/130: include in section "2.1 Study Watersheds" that the topography is similar and how that is assessed

5) Line 134: The third criteria seems to be excluded from the analysis. It is not mentioned after this section. If that is the case, then exclude it here too.

6) Line 158: It is unclear what exactly is meant with the "occurrence of a TI" and how it is determined (it's actually clearer in the caption of Fig 3)

7) Line 160: It is unclear what exactly is meant with the "contribution of each TI" and how it is determined

8) Line 167: Include results indicating that other (groups of) TIs are indeed repetitive and should be excluded for the sake of verification

9) Line 170: Variance of what? How is it calculated?

10) Line 170-175 and Fig2: SA is the surface area, yet it is not grouped in group 2 which describes the area!

11) Line 182: Confusing formulation of "1, 1, 4, 8, 9, 10 and 11"

12) Line 194: Sentence contradicts results in Fig 3. According to Fig 3 SA does not play a significant role in Qmin, but is significant in Q75%.

13) Line 221: "positive relationship between the selected TIs and low flow variables";

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this is not the case for the openness which always has negative estimates in the regression model results as shown in the supplements

14) Line 256: It is not surprising these commonly used TIs such as slope was not in the final list as it was excluded from the selected TIs and therefore its contribution was not even calculated in order to end up in the final list. Its contribution is only indirectly assessed through other TIs. How different would the results and conclusion be if all TIs were included?

15) Fig 1: hydrometric stations are plotted, yet not mentioned in the paper. It is suggested to either mention how they were included or exclude them from the figure.

Technical corrections:

1) Line 170 and 175: Fig 3 written, yet probably referred to Fig 2

2) Line 193: Fig 4-7 missing

3) Fig 1: missing scale bar for the small map of the state. This small map misses the background map showing the location of neighbouring land, now it seems the state is an island which is not the case!

4) Fig 2: missing description of axes (what do these numbers on the axes indicate?); confusing choice of words: factor 2 = group 2. Use the same thing in the figure and label.

5) Supplement tables list: inconsistent font types

6) Supplement Fig S1: inconsistent abbreviation for precipitation (PPT in figure and P in capture); do not connect the points to lines as the results for each watershed are independent from each other; line for temperature is not visible in a black/white print

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