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Hydrology and Earth System Sciences
Discussions

Interactive comment on "On the dynamic nature of hydrological similarity" *by* Ralf Loritz et al.

C. Harman (Referee)

charman1@jhu.edu

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Entropy over hillslopes or time?

This paper addresses a very interesting idea and is very well written and presented. I have only one major concern, but it is right at the core of the matter and addressing it may affect the interpretation of the results.

The main issue has to do with the calculation of entropy. My question, succinctly, is whether x in equation 2 refers to hillslopes or to timesteps? Or sometimes one, and sometimes the other? I have scoured the manuscript and find contradictory statements that could lead to both conclusions. The conflation between these seems to me to cast doubt on the assertion in equations 4 and 8 that the entropy can be used to determine the number of functional groups necessary to model the catchment in a non-redundant way, and that results suggest the ideal number is time-varying - major conclusions of

the paper.

The statement in Line 311 that Shannon entropy is calculated for each timestep suggests that it is being summed over hillslopes (one Shannon entropy for each timestep). That is, $p(x)$ is the marginal probability that a hillslope has a particular discharge at that timestep, and so $p(x)$ must be normalized such that the total probability at each timestep is 1. This is the entropy that is presumably being plotted in figure 4c and d.

However, if this is the case, the entropy would be maximized at any given timestep if the discharge from a randomly chosen hillslope has an equal probability of occupying any particular discharge bin. This conflicts with the definition given in equation 4. Wouldn't the maximum entropy be $\log_2(M)$, where M is the number of bins? Given that you have log-spaced bins starting at 0.01 and increasing by 8.5% each time up to a maximum of around 0.7 mm/hr, it looks to me like you have about 26 bins. That would give a maximum entropy of about 4.7 bits.

It seems important that 4.7 bits is slightly larger than the largest the entropy values you observe in Figure 4c.

However, there are other points in the paper that suggest this interpretation cannot be the case (at least not all the time). What would be the meaning of y in equations 3,5, 6 7? It would have to be the set of discharges at a different timestep from x. But then all the calculations would be about the mutual information between timesteps, and that is not what this paper is about. It seems then that x and y are now different hillslopes (one Shannon entropy for each hillslope). Now $p(x)$ is the marginal probability that a timestep has a particular discharge at in a given hillslope. Also $p(x)$ must be normalized such that the total probability across all timesteps for each hillslope is 1 (rather than across all hillslopes for each timestep). The p associated with a particular hillslope/timestep pair will differ depending on which marginal distribution it is being considered a part of.

This is the only way I can understand how it is possible to plot both a time-varying entropy in figure 4c, but also have a single invariant normalized mutual information (NMI) metric for each hillslope for use in the functional classification. It seems different marginal distributions are in play at different points in the manuscript, and they are not clearly distinguished.

I find myself doubtful that Equation 4 and 8 hold. Equation 4 does not seem to me to have the meaning implied, for the reasons I give above - maximum entropy occurs when all discharges are equally likely and has nothing to do with the number of hillslopes. Similarly, the entropy of a coin-toss experiment is maximized if the coin is fair, and has equal likelihood of landing on either side, and has nothing to do with the number of times I flip it.

This doubt then extends to equation 8. I find myself uncertain as to whether it is valid to interpret the entropy in figure 4c as saying anything about the time-varying number of non-redundant hillslopes required to simulate the result. If the theoretical maximum is indeed 4.7 bits, does that mean we know, even before we run the model, that we will need at most 26 non-redundant hillslopes? Is this how the uncertainty in the discharge immediately limits the required model complexity?

I wonder if a simple thought experiment (or numerical experiment) would help illustrate the validity of all this? One where we know what the 'true' number of non-redundant 'hillslopes' is, and it can be shown that this is recovered by the analysis?

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