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Hydrology and Earth System Sciences Discussions

Interactive comment on "Catchment classification: empirical analysis of hydrologic similarity based on catchment function in the eastern USA" *by* K. Sawicz et al.

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Response to Comments of Reviewer 3 Please see attachment for all figures associated with this response.

[REVIEWER] The authors present preliminary results to support the use of available streamïňĆow, temperature and precipitation data for classiïňĄcation of watershed (basin) function. The goal of watershed function classiïňĄcation is a current and active subject of research. The basic premise of this work, that existent hydrologic data can be used to provide clues for the underlying 'watershed classiïňĄcation' framework, is

original and is carefully tested using up-to-date methodologies of data analysis and classiïňĄcation techniques. Two conjectures (or hypothesis) emerge from their analysis: (C1) StreamïňĆow elasticity with respect to precipitation is modiïňĄed by the soil characteristics of a catchment. (C2) Spatial proximity is a good ïňĄrst indicator of hydrologic similarity because of the strong control climate exerts on catchment function, and because it varies slowly in space. Several concerns regarding the validity of the conclusions reached on this paper arise. First, the analysis presented mixes basins of different size. Although, a histogram of the distribution of basin areas is not presented (and it should be), it is clear from Figure 2 that basin size varies signiïňĄcantly between the watersheds being considered. The authors do provide the range of basin areas (67 km2 to 10,096 km2), but it cannot be deduced from the available ïňĄgures and results the role that total watershed area plays in the ïňĄnal classiïňĄcation results. There seems to be an implicit assumption throughout the analysis that the indexes used in the classiïňĄcation scheme are scale independent (or that they vary in the same fashion as a function of scale). My initial reaction is that this implicit assumption may be incorrect, rendering the two major conclusions of the paper (i.e. C1 and C2) invalid.

[AUTHORS] A histogram of drainage area for catchments in this study is presented below as Response Figure 1 for your consideration. We have chosen not to include this histogram in the revision of the manuscript because drainage area is not found to be a controlling property of classification as discussed in more detail below. The other issues raised are addressed below.

[REVIEWER] In order to support (and explain) the argument above consider the physical processes occurring at a ïňĄx spatial scale: the hillslope scale. Reviewer-3_Figure-1.png illustrates how two of the indexes (FQP and IBF), can be strongly affected by hillslope/bedrock shape. Here, similar changes in the water table levels (from H1 to H2) can lead to very different ratios in the partition of subsurface and surface runoff due to uneven changes in the saturated area (variable source area). Notice, that nothing prevents two hillslopes with such different shapes to occur nearby to each other. Making the conjecture C2 invalid at this scale. This example is only a conceptual illustration, but a more detailed study of the relation between the three dimensional shape of the hillslope/bedrock can be found in Troch et al (2003) and Hilberts et al (2004).

[AUTHORS] The reviewer raises an interesting point regarding the importance of hillslope shape on the overall watershed response and his point is certainly valid at the hillslope or small catchment scale. However, studies by one of our co-authors (Peter Troch), who is also a co-author on the papers cited by the reviewer, shows that hillslope shape is unlikely to exert a dominant control on any of the signatures used in our study even for the smallest catchments included (67km2). Even the smallest watershed (67km2) is too large to exhibit a hillslope shape control. Ongoing research by Troch's group focuses on geomorphological controls of hydrologic similarity and specifically addresses the issue of catchment-average hillslope width function and channel network width function controls on the hydrologic signatures used in our paper. In addition to the empirical analysis of this issue, we have a second parallel paper currently in review in HESS (Garrillo et al., HESS-D) in which we use a model-based framework to assess controls on the signatures studied. The model used is based on the Troch et al. (2003) modeling framework that the reviewer cites.

[REVIEWER] As watershed area increases, the number of hillslopes increases as well in a linear fashion (e.g. Gupta 2004), which tends to smooth out local effects, and watershed scale controls begin to dominate the basin response (e.g. soil types). What is the scale where those watershed characteristics begin to appear? This question is not addressed in this paper, nor it's demonstrated that the scales considered there are appropriate. But most importantly it is unclear that scale effects disappear at a given scale.

[AUTHORS] The size ranges of our catchments do not allow us to answer the question posed by the reviewer since even our smallest catchment is too large for this issue (67km2). However, there has been other work done in the past that does look at the cutoff between hillslope and catchment controls. It was for example shown by

Robinson et al. (1995) that hillslope processes only control streamflow characteristics at very small scales («10 km2). It is also shown that at approximately 10km2, larger scale geomorphological diffusive processes become dominant. Rinaldo et al. (1991) also discuss the combination of geomorphic (larger scale network) and hydrodynamic (hillslope) processes and modeling in great detail. This information will be added to the manuscript to address your comment.

[REVIEWER] Consider the indicator RLD used to quantify the 'ïňĆashiness' of a given watershed. This index provides a time scale (average slope) for the rising limb of the hydrograph. When comparing two basin of identical size, the index provides a good indicator of which of the two responds more rapidly to precipitation inputs. The index is strongly controlled by the topology and geometry of the river network and the spatial distribution of hillslope shapes. However, this index is not scale independent, which can create difficulties when comparing geographical distinct regions. In order to illustrate this issue, consider two basin of different size, thus having different concentration times (tc), and assume that the unit hydrograph for those two basins can be given by a triangular hydrograph (see Reviewer-3_Figure-2.png). For the basin with tc = 1 hr RLD = 2, while for the basin with tc = 10 hr RLD = 0.2. Even though, the two basin exhibit the same slope for the rising limb they exhibit different values of RLD. Thus, given that basins are a system of embedded watersheds, the class assigned based on this index would only apply to one scale on a geographical region. A similar argument can be developed for other indexes used in the analysis, that invalidate conjecture C2.

[AUTHORS] With respect to hypothesis 2 (C2), we do find that climate controlled signatures vary slowly in space, which is the argument that we are making here. This argument is not invalidated by the reviewer's statement made above, which focuses on RLD. RLD, as the reviewer states, is likely to be strongly controlled by the geometry of the catchment, rather than by climate. We did not find a scale dependence of RLD in our empirical analysis, though we further investigate some of these issues in the above mentioned companion paper by Carrillo et al. (2011). Heterogeneity of any data set limits the ability of an empirical study to identify controls. We therefore rely on physically based modeling to further assess the validity of our conjectures.

[REVIEWER] My ïňĄnal comment is in regards to conjecture C1. Here streamïňĆow elasticity with respect to precipitation is evaluated at the annual scale. Although, the index provides a good measure of basin response for large scale basins (> 10,000 km2) where annual precipitation totals provide a good indicator of rainfall over the basin, for smaller scales, timing and intensities of storm events can be masked by this index (i.e. A basin will react differently to 100 mm of rain if they fall over 24 hours or over 1 hour). Thus two years with equal value dP/P and have widely different values of dQ/Q. The measure EQP would be less robust for small watersheds than for large watersheds, where the effects of individual storm properties are smoothed out by the transit of the runoff on the river network (e.g. Rinaldo et al. 1991). I have conducted and independent study for the quantity EQP for basins in Iowa to determine the extent of the effect of scale and the validity of the conjecture C1. In this geographical setting. My results reveal values of EQP varying from -1 to 5, changing in a very random fashion from basin to basin and with very little geographical consistency, and no apparent correlation with soil types in Iowa. Also the results show more variability of the quantity for smaller basin than it does for large basin. Since, the manuscript does not report the range of values found by the authors (only the histogram of the normalized values are given) it is difficult for us to determine is the range of variability found in Iowa basins is small or large relative to the results observed for the dataset used by the authors. The ïňĄle Reviewer-3_Figure-3.png shows the variability of the index as a function of area.

[AUTHORS] Response Figure 2 shows the change of storm intensity over time for all catchments. The result shows an interesting cyclical trend, but the overwhelming number of intensities vary between 1.2 and 0.8 (value of 1 representing the mean). Due to the fact that catchments generally follow this cyclical behavior together, the effect of storm intensity between catchments is inconsequential. To further demonstrate this point, Response Figures 3 and 4 show the mean and variance of storm intensity, how-

ever no significant correlation between either of these climatic propeties are found. The argument that we use to support conjecture 1 is based on the classification result. The grouping identified suggests that variability in soil characteristics plays a role. This will have to be further tested in the physically-based modeling framework discussed in the companion paper by Carrillo et al. (2011, HESS-D). With all empirical studies, a large number of data points are needed in order to suggest a conjecture or hypothesis for future testing. However, these statements are only valid for the range of conditions that the data provides (not generalizable within the empirical study itself). Therefore, the conclusions reached in this study are possible hypotheses that can be used for further analysis, and are presented as such in the manuscript. These hypotheses rely on a large number of physical property combinations, which is provided by the dataset use for this study. It is not clear from the reviewer's comments what method was used to calculate EQP. The period of time used to calculate these values was not also included. The signature EQP is sensitive to the way it is calculated. Without ensuring a similarity in method, it is difficult to comment on the reviewer's findings. However, as stated in the previous paragraph, for any empirical study it is the variability of characteristics in the dataset that controls much of the result.

[REVIEWER] The current manuscript can be improved by addressing the issues raised above, and the authors are encouraged to describe in a more through fashion the underlying physical mechanisms described by selected classiïňĄcation indexes. The validity of the conjectures reached on this paper are pending on a detailed analysis of scale independence of the indexes in consideration. This issue needs to be clearly assessed and demonstrated before this work can be accepted for publication.

[AUTHORS] With respect to the underlying physical mechanisms related to the different signatures used, we previously discussed them in greater depth, but initial review comments by colleagues suggested that much of this description was superfluous given the widespread use of most of the signatures. We therefore decided to reduce the length of this part of the paper. However, we will revisit this issue in the revised version and probably add some of the information back in as suggested by the reviewer. Regarding scale independence of the signatures used, we did not find any correlation between catchment size and signature values. Hence, we did not focus on the scale issue for this paper. There might of course be other signatures that show scaling behavior, but this was not the case for our dataset.

REFERENCES Robinson, J. S., M. Sivapalan, J. D. Snell. 1995. On the relative roles of hillslope processes, channel routing, and network geomorphology in the hydrologic response of natural catchments. Water Resources Research. 31. 3089-3101.

Rinaldo, A., Marani, A., and Rigon, R. 1991. Geomorphological Dispersion. Water Resources Research. 27. 4. 513-525.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/8/C3117/2011/hessd-8-C3117-2011-supplement.pdf

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