

***Interactive comment on* “Similarity between runoff coefficient and perennial stream density in the Budyko framework” by D. Wang and L. Wu**

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Drainage density is a very important geomorphology and hydrology parameter for terrain dynamic and hydrologic simulation and has significant values in practical water management. An alternative way to define drainage density is to identify the locations of headwaters where water channel starts, which however is very challenging by field work, remote sensing or theory analysis methods. Although with a short presentation, the authors creatively found the similarity between the perennial drainage density and the complementary Budyko curve—which essentially presents runoff ratio.

As the increase of desires for hydrology information (e.g. floods) at higher temporal and spatial resolution, at which high quality meteorological forcing data are getting

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available (e.g. TMPA, CMORPH, and GPM/IMERG etc), I would consider temporal drainage density is in more of desire, as it is very useful in delineation of subgrid scale routing, and may have significant impacts for high temporal (e.g. 3 hourly) river basin routing calculations. However, the results for perennial rivers shown by the authors are very encouraging, providing a very good basis for further investigation and derivation of total drainage density.

Many studies have been conducted to relate drainage density to variable factors, e.g. precipitation, P-E, extreme events (floods). However, the authors results demonstrate runoff ratio is a more reliable indicator for perennial drainage density, which I think is promising as runoff generation/routing should be the first order, in most areas, at annual scale in formulating channel. As an integration process of partitioning precipitation, runoff is the factor which directly interacts with land surface, e.g. by erosion.

The ms can be published as current form. But I have a few questions and suggestions for the authors with the hope it would be helpful for the discussion and the authors future work.

(1)The authors response to Dr. Sivapalan is very helpful and interesting. Regarding to the U shape in figure 1 in the response, I generally agree with the authors as it seems to conform to the common sense. Vegetation facilitates infiltration and impedes erosion, while it may lead to higher subsurface slow flow. Therefore, the vegetation would somewhat twist the drainage density/precipitation curve leading to a sort of U-shape. However, I am wondering if the vegetation function will contribute more to perennial rivers at annual scale, while at the widespread upstream areas, the impacts could also lead to more temporal rivers at seasonal/monthly scales as response to precipitation events, depending on the topography and lithology characteristics at hillslope scale. So I am not sure if it is appropriate for authors explanation by the trade-off of runoff at different time scales (maybe more appropriate using “runoff component”?) and be deferred from the dominance of temporal or perennial river density. The temporal rivers are mostly located in widespread upstream areas, with a much higher proportion to

total river length than that of perennial rivers. Actually, I am wondering if the temporal drainage density would widely dominate the curve shape between the total drainage density and arid index, i.e. the combination of two curves in the last second box may not lead to a U-shape in the bottom box in Figure 1. The dominance by temporal rivers can be seen from the 5 cases shown in Figure 9-12. Even in the case of Fig. 8 where the basin is well snowmelt and groundwater dominated shows a half to half perennial and temporal river length ratio.

(2)What do the authors think about the scalability of the approach describing the relation between perennial drainage density and runoff ratio? If the approach is pretty scalable, I would suggest a self-validation method by checking the relationship within a relative larger basin while hierarchically applying the approach within the basin, i.e. applying the approach to all the most upstream sub-basins (i.e. first order basin) will lead to the location of headwater cell, which actually will also determine the drainage density of the entire basin; then compare the determined drainage density with that derived from the approach based on the entire basin. The comparison can be conducted on the drainage length, which is directly useful for hydrologic modeling. This may be a way to avoid collecting the measurements of stream length which are scarcely available at global scale, although they should still be desirable. By this some interesting experiments can be done in a relatively convenient way, for example, comparison between two regions with similarities, e.g. Norway has almost same precipitation as Congo, while ET in Norway is significantly limited by energy and both areas have high vegetation cover. What would be the difference in the drainage density between the two areas? Will the findings in the experiment strengthen the authors statement? I would be very interested in seeing that. One concern here is the uncertainty in precipitation data may tend to decrease as drainage basin area increases, which may lead to some uncertainty in the analysis.

(3)Why P-E is less reliable in the relation to drainage density than runoff ratio? Does this indicate the uncertainty in precipitation data? For this question, I am also won-

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dering whether a well calibrated/validated hydrologic model can help (at least give feed-back to) the drainage density derivation. Can the approach proposed by the authors be combined with a hydrologic model to formulate a way to define a spatially distributed runoff accumulation threshold map for identification of headwater. Again drainage length defined by this way can be more useful for hydrologic modeling.

minor revisions:

#1. L3-7/p573: it could be rephased to be clearer.

#2. statements on runoff function are repeated a few times.

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