

Interactive comment on “Integrating network topology metrics into studies of catchment-level effects on habitat diversity” by Eleanore L. Heasley et al.

Anonymous Referee #2

Received and published: 10 March 2019

The authors try to quantify the effect of network topological metrics on habitat diversity using examples of four diverse catchments in the UK. The idea is interesting, and the paper is written well. However, there are some major issues in the paper that need to be addressed before publication. Thus, I would recommend major revisions, and my main comment related to bullet 2. Following are my major and minor comments:

1. The authors claim in the abstract, and then in the Introduction, that they have developed two new network metrics – however, I struggle to see how the metrics are new. The distance network density is the same as the width function, while the elevation network density is the same as the link concentration factor. The authors say that they

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have adapted these metrics, but all I see is that the authors have used the metrics. Adaptation of the metrics would involve changing them in some specific way, and this has not been done to my understanding. If this is not correct, the authors need to clarify. However, if it is correct then the authors need to be clear that the development of the metrics is not the contribution of their research..it is rather the application of the metrics. 2. The authors state in the abstract "The results indicate that the new metrics offer a richer, and functionally more-relevant description of network topology than stream order, highlighting differences in the density and spatial arrangement of each catchment's internal network structure". However, when I read the paper I struggle to see that the evidence really points to this statement. My understanding is that this statement is based on Figure 4. However, figure 4 does not really show this effectively. The authors do correlations with the network topology metrics, but there is no correlation attempted with stream order, which make it a difficult comparison to make. Moreover, the correlations with the new metrics are weak and most of them are non-significant. This makes it an extremely difficult argument to make. The novelty of this paper is that these new metrics are better – however the evidence that the authors present does not convince me of this. 3. One suggestion for figure 4 might be to attempt to correlate median and 1st and 3rd quartiles, instead of mean, max and min. Max and min values can be highly erratic with environmental data, and might not always be amenable to such analysis 4. Line 410 – this is extremely qualitative – this section should focus on the authors findings, but instead becomes more of a lit review..and no substantive reason to argue for a lack of reason. Is there a p value and r2 for one set of correlations that is better than the other? 5. Line 380 – why does increasing network density lead to reducing minimum habitat diversity but increasing max habitat diversity? The arable land explanation provided here is not clear The metrics should be described in the abstract to make the statements made in the abstract clearer.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2018-89>, 2018.

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