Response to referees' comments on "Integrating network topology metrics into studies of catchment-level effects on habitat diversity" by Eleanore L. Heasley et al.

#	Referee	Comment	Response	Lines (new version with track changes)
1	1	Sections 1.1 and 1.2 should be merged together. Many concepts present in Sect1.1 are repeated in Sect1.2 and I don't find a clear division between introduction to "Network topology" in Sect. 1.1 and its "effects on in channel functioning" in Sect 1.2 as suggested by their title.	**Part-addressed:** We argue that there is a clear distinction between the content of these sections with Section 1.1 focusing on the different dimensions of the river network and methods to quantify it, whereas Section 1.2 focusses on the geomorphic and ecological response of reaches to network structure. However, we see that the sub-titles could make this clearer and so have changed the title of Section 1.1 to "Quantifying the river network at different scales and dimensions"	Line 90
2	1	Sect 1.2 focuses extensively on confluences but the reader yet do not understand why they are so central in the network topology metrics and you need to clarify if the metrics proposed describe primarily how confluences are spatially organized or also other network features.	**Addressed:** A paragraph dealing on the effects of confluences on specific RHS features has been moved to the methods section. This move was made to (a) justify why those specific RHS variables may be impacted by the network and (b) make Section 1.2 more network-focussed than the impacts of individual confluences	Line 267
3	1	Section 2.1 start with few sentences (lines 170-175) which sound more as paper objectives and for this reason should be moved into the introduction. Here, I suggest revising lines 70-78 to clarify paper objectives.	**Part-addressed:** We agree that the first sentence of Section 2.1 sounded more like an objective and so have combined it into the previous section. The remainder of the first paragraph describes the rationale behind choosing the study sites and so is retained in Section 2.1	Line 184
4	1	The scoring system adopted is not robust. It simply evaluates 'as better' more frequent presence of specific features. This is a very subjective criterion, which cannot be linked easily to any geomorphological processes associable to various network topologies….	**Addressed:** We have added correlations for the individual components of the original scoring system (flow type and sediment size) as these are less subjective than the original scoring systems adopted by the RHS dataset and can be more easily linked to geomorphological processes. The HQA and HMS scores are retained, however. This is because, although HQA and HMS scores are more subjective as Referee 1 points out, they are overall quality measures used for regulatory compliance and so network topological impact on overall 'score' is relevant (this is explained in Methods Section 2.3 – Line 257).	Figure 4
5	1	… Ideally you should encourage the adoption of more processes based river channel classifications, where morphological forms are classified based on different channel processes and then can be associated to different network topologies on the basis of physical principles (suggested literature). The discussion sections should dedicate a paragraph to address these limitations and	**Addressed:** This limitation has been included in Methods Section 2.3 while recommendations for better classification schemes are addressed in Discussion Section 4.2.	Line 262 and 432

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<tr><td></td><td></td><td>propose future applications capable of balancing the good availability of RHS data (in case of UK) with the need of better classification schemes.</td><td></td><td></td></tr>
<tr><td>6</td><td>1</td><td>In the result sections you often comment how the presence of anthropic pressures may alter the statistical correlations analysed. This is a good point. However, RHS database does includes quite an elevate number of features regarding in-channel artificial features. It would be very interesting to see if an indicator of artificial channel degree extracted from the RHS DB is significantly correlated with the unexplained variance of your model. This would be a notable finding, and also a good use of the RHS variables.</td><td>Addressed: Correlations between network topology and HMS Score have been added to the paper (see response 4-1 above)</td><td>Figure 4</td></tr>
<tr><td>7</td><td>1</td><td>RHS is sampled on reach of 500 meter in any river (and it is not suitable for large rivers). For this reason the frequency of features used in your scoring systems of habitat diversity is likely deeply affected by river channel size (bigger river have larger river channel features). This limitation should be commented. You should try to see if there is a bias in your statistical analysis associated with basin area.</td><td>Addressed: To address this, we have added a paragraph in Section 4.2 to describe this limitation. However, there are few significant correlations (see Figure 4) between stream order (which is strongly correlated river size) and RHS features, suggesting that river size may not influence RHS variables as much as hypothesised in the case of these catchments.</td><td>Line 438</td></tr>
<tr><td>8</td><td>1</td><td>Would be nice to see how the ability to predict habitat diversity (though the limitation of the metric used) improve using all the three indexes, i.e., a multivariate regression. This may help to explain if the indexes are mutual exclusive, or if each of them explain different aspects of habitat diversity. This may help to disclose the physical link between network topological features and habitat diversity. In case you don’t want to develop this additional analysis, you should justify in the discussion why you don’t think this is necessary and debate this issue.</td><td>Part-addressed: The use of the network topology metrics in predicting RHS data is not an objective of this paper so we have not conducted a multivariate regression as suggested. Rather, our aim is to explain how network topology influence river reach characteristics. However, Referee 1’s point about whether the three metrics are mutually exclusive is important for our aim. Consequently, we calculated correlations between the three metrics with results below:
<table>
<tr><td></td><td>Distance Network Density</td><td>Elevation Network Density</td></tr>
<tr><td>Elevation Network Density</td><td>tau=0.17</td><td>-</td></tr>
<tr><td>Stream order</td><td>tau=0.03</td><td>tau=-0.03</td></tr>
</table>
From this, we conclude that each metric reflects a different aspect of the river network, as the correlations are not strong (all with Tau<0.2). This is described in prose in Results Section 3.1.</td><td>Line 340</td></tr>
<tr><td>9</td><td>1</td><td>Lines 141: “For example, substrate size changes from coarse to fine downstream along each “sedimentary link””. Yes but sometime also fine to coarse. Please revise.</td><td>Addressed: In Rice et al.’s (2001) paper they report downstream fining along sedimentary links with step-like increases in sediment calibre at confluences. This has been reworded to make it clear that we</td><td>Line 156</td></tr>
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			are drawing on Rice's work directly rather than downstream fining more generally	
10	1	Lines 159-161 are unclear to me, please revise.	**Addressed**	Line 169
11	1	Lines 353-357 are unclear to me, please revise.	**Addressed**	Line 417
12	1	Lines 360-361 "This may be because each distance interval contains a broader range of elevations than elevation intervals within which elevation range is controlled. Impacts of network topology on instream physical habitats". Very unclear to me, are you saying that elevation is more important because it varies more in the 'distance' metric? If true, this would be a strong statement which affect your results and the meaning of your indexes and should be much more extensively debated.	**Addressed:** This paragraph has been restructured and this point combined into the previous paragraph. The new results show that the distance metric in not better (in the sense of number of correlations) but that it represents a different component of network topology compared to elevation network structure and thus has different responses (Fig 4). We have tried to clarify this point throughout the discussion.	Line 423
13	1	Lines 428-431: lack of correlation does not depend only on channel energy but also on network forms, indeed Wensum has the most "linear" shape. Can you better explain this point?	**Addressed:** We no longer make this point due to the positive results in all catchments for at least one RHS variable.	-
14	1	Lines 445-447 ("The study also highlights that anthropogenic modification and other factors mean that network density only has an impact at some sites in the catchment".). You cannot state that. Your findings did not prove that, since you have no quantitative information on the level of anthropic pressures in your sites (see my suggestion to add this information from the RHS database). You can presume it based on previous literature. Please revise this point.	**Addressed:** The HMS score has been included in the study to improve this statement. However, it is still not possible to make this statement conclusively based on the analysis conducted, so this section of the conclusion has been reworded.	Line 624
15	1	Lines 447-448 ("This paper shows that network topology itself may be in-part influenced by catchment characteristics. . ."). When? How? It is unclear to me. Please explain.	**Addressed:** As above, we agree with Referee 1 that this this is too bold a statement to make given the analysis conducted. This sentence has been rewritten and a suggestion for further research to address this has been added to the Discussion Line 603.	Line 624
16	1	Appendix A is never cited in the main text.	**Addressed:** Appendix A no longer necessary and has been removed from revised paper.	-
17	1	Caption of Figure 4: "Significant correlation coefficients ($p<0.05$) shown in bold" should refer to (a) and not (b).	**Addressed:** No longer relevant – Figure 4 caption altered (see Additional Changes #8)	-
18	2	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 have adapted these metrics, but all I see is that the authors have used	**Addressed:** We agree with Referee 2's point that the networks are not 'new' and the terminology in the abstract and introduction (and any other points in the paper) have been altered to highlight the novel application rather than the new development of the metrics for clarity.	Abstract and Intro

		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.		
19	2	…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….	**Addressed:** Stream order was originally treated as a categorical variable. However, correlations have been conducted as requested to make comparisons between topology metrics more straightforward. To do this, the correlation method was changed from Spearman to Kendall for all correlations to account for the tied data introduced by the stream order variable, as Kendall is less sensitive to tied data than Spearman's. See Section 2.4.1 for description of the revised statistical methods	Figure 4
20	2	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.	**Addressed:** The strength of the correlations does vary. The results, however, are still interesting as they help identify relationships that have not before been explored between network topology and river characteristics nor with the broad-scale monitoring-type data we use. While they may not be strong, the correlations show clear differences in effects of network metrics between catchments and that the network density metrics have more significant correlations than with stream order both of which are interesting and novel results. This suggests that the network density metrics are better at functionally representing network topology in these catchments.	-
21	2	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	**Part-addressed:** We agree that Referee 2 is right that max and min values can be erratic in environmental data, however, we argue that using 1st and 3rd quartiles will mute the extremes that are often critical for environmental function (e.g. see *Junk, W. J., Bayley, P. B., & Sparks, R. E. (1989). The flood pulse concept in river-floodplain systems. Canadian special publication of fisheries and aquatic sciences, 106(1), 110-127*.). Therefore, we have replaced the max and min with 90th and 10th percentile variables to reduce influence of anomalous extreme values while still characterising the tails of the data distribution.	Figure 4
22	2	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	**Addressed:** Section 4.3 has been rewritten to discuss the results quantitatively and refers to the relevant figures.	Line 561

		for a lack of reason. Is there a p value and r2 for one set of correlations that is better than the other?		
23	2	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.	**Addressed:** This comment has been removed in the new discussion Section 4.2 (see Additional Changes #7)	-
24	2	The metrics should be described in the abstract to make the statements made in the abstract clearer.	**Addressed:** The metrics are better described in the abstract	Line 13

Additional changes:

#	Description	Justification
1	Catchment maps from Figure 3 moved to Figure 2.	This avoids repeating the elevation data and allows the images to be larger for the benefit of the reader.
2	Figure 3 now contains distribution of the network density metrics and RHS variables	It is useful to the reader to directly compare the metrics to the RHS variable in question. The loess lines are included to better visualise the trends discussed than the points alone.
3	Distribution of number of stream order links in Figure 3 removed	The number of links in each stream order is not the metric explored in this study, so it was removed. The distributions are all similar and unsurprising and are described on Line 339.
4	Table 1 altered to include all RHS variables explored in the paper	Detailed description of HQA and HMS analysis is referenced (Line 257) rather than explained in-depth in the text.
5	Section 2.3 altered to description of RHS variables used in study	A description of each variable and literature on how it responds to confluences is included
6	Results Section 3.2 rewritten	Section had to be rewritten to account for the analysis of the new variables
7	Discussion Section 4.2 rewritten	Section had to be rewritten to account for the analysis of the new variables
8	Figure 4 altered with new analysis	Figure 4 now includes results of all the new correlations conducted. It was impractical to present each correlation as a line graph like the original Figure 4, so a grid format was more concise and makes it easier to see trends