Author Response to Referee Comments

William Booker and Brett Eaton

We would like to thank both referees for their time and comments. The concepts raised in their reviews will substantially improve the quality of both the ideas and their written communication. We have addressed the referees' comments on the substance of the paper below.

RC1:

[Comment] I appreciate the opportunity to provide a review and commentary on the manuscript
"Morphodynamic styles: characterising the behaviour of gravel-bed rivers using a novel, quantitative index" by Booker and Eaton. This is a very well written manuscript that introduces an index of channel process that I've thought about for a long time - but unlike the authors here, I've been unable to put it into numerical terms so elegantly! Well done in that regard.

I believe that the methods developed in this paper will aid other researchers in linking form and process in river systems and in interpreting their own sediment transport and topographic change data together using the throughput index. This paper should generate wide readership and will be of interest to those who follow ESurfD.
In the attached PDF document, I've asked the reviewers to (a) refine the introductory text slightly to acknowledge links between form and process present in many channel classification schemes, (b) clear up some details in the methodological explanations, (c) make slight figure edits, and (d) more fully incorporate text detailing the potential for errors in either sediment transport or DEM/DOD change detection to influence the throughput index. There are also numerous text edits which are minor in nature. Taken together, I believe these modest edits will make what is already a well-written paper even stronger. Thanks again for the opportunity to review this manuscript!

[Authors] We would like to thank the reviewer for their help in improving the manuscript; the comments provided here have given great food for thought regarding the overall methodology and how best to communicate it.

[Comment] I see the point here, but I'd suggest softening this language if possible; perhaps use "may not fully capture the feedbacks"? At least in the case of Wilcock (1993), the author indeed relates sediment transport to channel morphologic units (see text beginning last paragraph of page 501).

[Authors] We agree that this statement can be made more inclusive of the literature on form-process interaction and will amend the text to reflect this.

[Comment] I suggest leaving this sentence at "This hierarchal style of classification has been widely used within the river management community" (or some variation thereof). There are many who would argue that NCD is neither light on fieldwork nor easy to use, especially at the more advanced levels of classification (III/IV), and diverting into this debate might distract from the main focus of the paper.

[Authors] Given that this paper is meant as supplementary to other methodologies, we agree that unnecessary distraction of other classifications should be avoided.

[Comment] I'm having trouble intuitively categorizing/binning the items named below into these two axes as the text is written. For the first axis, there are things like vegetation, jamming, and bifurcation (the latter of which is often the result of the first two, right?), and then for the second axis, there are channel evolution and antecedent conditions? At least as written this list of items doesn't fit nicely into two categories in my mind. Perhaps a way of simplifying this would be to say that channels differ in their boundary conditions (sediment and water supply) and then secondarily in their in-reach morphology and structural elements (wood/rock jams, bifurcations, bank cohesion, etc.). I'm not sure if that interpretation is in line with what the authors are thinking here, but I'd suggest reworking this text to be more intuitive.

[Authors] Improving the clarity here is paramount, because this section is a microcosm of the point of the paper. In the discussion of form and process it is our view that process should be the primary delineation used when considering channels. To that end, it is the inclusion of new processes that give rise to differences in character; contrasting a reach without cohesion to one with clay or vegetation and we observe a vastly different style of deformation. Thus these differences we believe should be considered as a type of boundary condition, in the same vein as one might consider supply or flow regimes. To that end we will rework this section to better represent the primacy of process in our framework.

[Comment] I don't think this term is very widely used, so I'd suggest clarifying with something like "jamming by boulders or large wood".

[Authors] We shall change the text to better convey our meaning.

[Comment] I'd caution that form and process aren't mutually exclusive, and oftentimes the metrics captured by form-based classification frameworks (things like channel sinuosity, slope, geomorphic units present) are indicators of the ability of a given channel to perform

geomorphic work - that is, form often reflects process, apart from channels which are severely out of equilibrium (for example, see https://journals.plos.org/plosone/article?id=10.1371/journal.pone.0150293). I suppose what I'm getting at is that it's not an either/or proposition, where one can only measure form in the absence of measuring process. I'd suggest making the case for an increased focus on process (as that is definitely an area for improvement in our understanding of fluvial dynamics), but also acknowledging that process can often be derived from measurements of channel form.

[Authors] We appreciate the caution of overstating the simplicity of form-based metrics, especially when there are metrics that implicitly include process as you have mentioned. Although this paper is attempting to shift the focus towards using process based analysis it uses a process-from-form abstraction. As such, it is dangerous to suggest that form measurements are simply geometric and we shall endeavour to clarify this viewpoint in the text.

[Comment] Does the text "as transport paths evolve" capture those cases where bars migrate or average bar spacing shifts through time? I'd think that either of those would affect the relationship between erosion and deposition sites.

[Authors] The evolution of transport paths here means a change in the character of transport length over the course of a single flow as feedbacks between sediment transport and morphology emerge based on the two trajectories shown in the experimental data reported here. In the case of bar migration or deposition zone relocation, the relation between erosion and depositional sites would shift in space. This would occur in an asynchronous manner with respect to other bars in sequence resulting in local variation in wavelength and character. Originally we had not considered this style of change but it would include cases of that origin as well.

[Comment] I'm a bit confused here, since Qb actually wraps Qbi and Qbo into one value; how is this done? Is it just the absolute value of the difference between them? I'd suggest expanding the Qb term here to be more explicit about how it incorporates both Qbi and Qbo.

[Authors] The Qb term is simply the mean volume of sediment in transport [(Qbi + Qbo)/2] as a potential perturbation of the system, to provide an average character of the volume of transport. To clarify this we will include the calculation of the Qb term.

[Comment] This text is difficult to understand as written, and I think it's sufficient to simply say that these are repeatable, fast, and reliable methods for capturing the bed surface.

[Authors] We will clarify the text here.

[Comment] It's unclear to me why this would be expected to be evenly distributed, especially given the fine-scale variability in sediment transport in natural rivers. Instead, I'd suggest phrasing this as something like "an idealized constant distribution of M values".

[Authors] The expectation of a type of distribution is a clumsy wording of the intent to produce a basic spatial representation of these values, we will clarify this in text.

[Comment] Keep the tense consistent throughout the results, note that its past tense here but present throughout much of the remainder. I think it reads better if it's all in past tense (since the experiments have already occurred).

[Authors] We will adjust the passage to maintain a consistent tense here.

[Comment] In the figures, please label the top and bottom DEMs in each panel as the fixed and mobile bank experiments.

[Authors] We will amend the figure captions.

[Comment] Shouldn't the mean of all the values be consistent through time (constant, in fact), unless this is a moving average or something to that effect?

[Reply] You are correct, this statement was meant to indicate a weak linear relation with respect to time. We will clarify the text to reflect the time variance.

[Comment] Please also articulate the variable width of the bars. As I understand it, these are showing the downstream variation in m, but it may not be immediately obvious (and I might also be mis-interpreting).

[Authors] We shall clarify the caption to reflect its representation of the full downstream variation in m for each timestep.

[Comment] This may have been mentioned in the text, and if so I apologize for overlooking it, but I'm a bit confused on how the throughput ratio is calculated continuously in the bottom panel. Doesn't the throughput ratio require a measurement of sediment into and out of a reach of interest, and here isn't that reach a single cross-section of pixels? Was a constant value of Qb used throughout the flume (as measured at the upstream and downstream boundaries)? I guess what I'm asking is if the authors can more clearly explain how a metric

that uses measures sediment influx and efflux can be calculated continuously along the flume, since obviously influx/efflux wasn't measured at each of the many, many cross sections here.

[Authors] Your interpretation of the boundary flux conditions is correct, although it may be possible to create a local calculation of sediment flux to calculate a true continuous throughput ratio we have not yet fully investigated it. Here, however, this methodology uses equation 5c, whereby a constant distribution of sediment transport is used to show how the local variation in morphologic activity affects the calculation of the throughput ratio. This uses the local ratio of observed to expected morphologic activity to adjust the reach averaged throughput ratio- which is why it is essentially an inversion of the distribution of M values about the mean throughput ratio (dashed red line). We shall clarify the origin of the calculation for the figures.

[Comment] The agreement (or disagreement) between (b) and (c) here is really interesting, and I hope it's something that will be brought up in the discussion. As I understand it, the places where (b) and (c) agree (that is, where b is strongly positive or negative and c is strongly positive) are those places where change was strongly erosional or depositional. Where c is large, but b is near-zero, those were places where a good deal of compensatory/offsetting geomorphic change occurred. Is that correct?

[Authors] Those areas of agreement/disagreement are exactly that. The patterns of M and net change across a cross section are not directly coupled and instead may reveal compensating morphologic change or one-sided erosion, for example. The spatial aspect of using the throughput ratio is of particular use in trying to identify differential zones along study areas such that we could define transport and storage zones (as in Desloges and Church) using a numeric basis.

[Comment] It's true that sediment transport is trending towards zero, but zero sediment transport isn't the end-game here, is it? That is, won't the channel arrive at some low-but-not-zero "equilibrium" sediment flux, assuming some sediment is introduced from upstream? If that's reasonable, I'd phrase this instead as being "a tendency toward decreasing and eventually stabilizing transport"

[Authors] It is unclear what the end state of the bed will be, given the potential disconnect that may emerge between the upstream and downstream sections of the experiment. However, we will change the phrasing as it is likely that there will remain a low level of activity that would preclude a fully stable morphology.

[Comment] One additional issue that came to mind here is the uncertainty or error in both the direct and remote sensing methods of sediment flux. That is, sediment traps aren't 100% accurate and DODs certainly contain some level of uncertainty or noise arising from constituent DEM errors. I'm wondering if the authors could comment on the potential for errors in either Qb or DEM-derived volumetric change to influence the throughput ratio? One option here would be to add some sort of error bars/error envelope onto the plotted points in F6, but that might require an amount of analysis that's beyond the scope of this paper. It would, of course, be very interesting nonetheless. That said, at a minimum I'd at least ask the authors to give some thought to measurement error/uncertainty and how it might influence the throughput ratio developed here.

[Authors] As pointed out by the other referee, we need to perform some uncertainty analyses to bound our data. With this in mind, we will include error analysis to provide limits on the throughput ratio. We expect error may become substantial as volumes decrease over the course of each experiment and increase the potential error range for these smaller denominator calculations- especially when qb values remain high, as in the fixed bank experiments.

RC2

[Comment] This manuscript presents an approach to analyse channel behaviour, using a new index
(the throughput ratio), from repeat, high-resolution topographic data. Such data are becoming increasingly attainable, as the authors note, not only in the laboratory but also potentially in the field. The manuscript thus makes a timely contribution and is likely to be drawn upon by an increasingly large number of geomorphologists who are generating, or have access to, such datasets. Figures are produced to a high quality and the manuscript is mostly written to a high quality. A recommend that a series of revisions are made, as listed below. Most of these are minor in nature but the consideration of uncertainty in the DEM of Difference analysis may require some further technical analysis to verify the results.

[Authors] We would like to thank the referee for providing their time and highlighting some very key improvements that will be made to the paper.

[Comment] Can you give more detail to the reader on the “implications” that will be discussed?

[Authors] The implications here are meant as the interpretation of results as it pertains to the differences between the systems, and the use-case limitations of the methodology for data availability. Both of these points are in the discussion but are not properly conveyed through the structure; we will amend the text to more clearly signpost these ‘implications’ for use.

[Comment] L88-99. I found the explanation in these paragraphs difficult to navigate. I think the expression could be improved with consistent terms e.g. “reaches” and “zones” / “wandering rivers” and “wandering channels”. If there is a reason for their difference then please explain the terms. Also, the framing of the difference between transport zones and storage zones was confusing with the insertion of “in contrast (then reference”; L94), which at first reading made me think that an alternative hypothesis had been presented. I recommend some restructuring. Finally, the final sentence on “hazard severity” is rather isolated. I don’t think you return to this theme but it could be part of the implications, in the discussion. At present, this comment on societal relevance of the research is rather “lonely” within the broader manuscript.

[Authors] We will endeavour to simplify the text here to better, and more consistently, convey the intention of this methodology. The changing use of terms is just an avoidance of repetition, not of semantic origin, and will be removed to avoid confusion. The inclusion of ‘hazard severity’ is at present vestigial and we shall alter our discussion to reflect its earlier inclusion.

[Comment] It appears that there has been no DEM of Difference uncertainty analysis applied. However, such methods are now widely established (e.g. https://gcd.riverscapes.xyz/). I recommend that the authors undertake an uncertainty analysis to verify their results.

[Authors] Thank you for bringing this up and providing a very useful tool for this analysis. As mentioned in response to the other referee, we will conduct an uncertainty analysis to bolster our results.

[Comment] Figure 2. This is potentially a very powerful illustration of the methodology but it is too
full of acronyms to be understood by itself with searching for definitions in the text.
This could be made more accessible to a general audience, who may look through the manuscripts figures before reading the text in detail.

[Authors] We will reorganise this figure to be more independent of the text and inaccessible acronyms.

Authors' Changes to Manuscript

There are four main categories of changes that we have made to our manuscript. The first is small scale spelling corrections and changes to help improve sentence structure, which are located throughout the document. Secondly, we have updated figures and figure captions on both the advice of the referees and due to an error in the original code (an extra term was included for the calculation of the throughput ratio in Fig. 6) that resulted in changes to the relevant text in 'Results'. Thirdly, the inclusion of a subsection on uncertainty that was requested by both referees also resulted in changes to Fig. 4-6 but are predominantly visually identifiable in Fig. 6. Finally, sections of the introduction and discussion were rewritten to both address referee comments and provide greater clarity on the structure of the paper.

The most substantial changes are listed below. Line reference refers to the position within the following track changes document.

L40-53: Reorganised to focus on availability of process as the determinant of behaviour using a comparison of simple and complex river channels.
L101-13: Simplified structure to better convey relevance to Desloges and Church (1989).
L160-76: Added section explaining how uncertainty was estimated for surface products and sediment output data.
Figure 2: Expanded definitions and clarified workflow within figure text.
Figure 4-6: Added bars representing uncertainty in reported and calculated values.
Results: Throughout tense was corrected to past and values changed where appropriate due to updated data processing. Where interpretation has changed (L245-53) text has been rewritten or removed.
L319-22: Added clarification of relevance to previous Desloges and Church (1989) comments.
Figure 8: Changed value names and description to better identify the source of this data.