This comment is in response to anonymous reviewer 2 (RC2).
Thank you very much for taking the time to thoughtfully review our manuscript. We have worked to carefully consider all of your proposed suggestions and revisions. Below we will give responses to the specific questions.

Thank you for the opportunity to review this manuscript. The manuscript uses a signal processing approach to estimate a representative path length for reach-averaged sediment transport estimation from a DEM of Difference. The topic is interesting and suitable for Earth Surface Dynamics. I am impressed with the method proposed in this paper. I believe it to be creative and containing the potential to explore sediment transfers from a DoD; however, I believe that this paper is currently unsuitable for publication. I believe this for several reasons.

First, the paper does not adequately provide sufficient background about the chosen topic. For example, it does not meaningfully define a path length, nor does it engage with the complexity of reducing a distribution of path lengths to a single number (i.e., "a characteristic path length"). The discussion of morphologic methods for bedload transport estimation and other methods to estimate path lengths from a DoD should also be expanded in the introduction. This lack of background information prevents the paper from making a sufficient case for why an abstracted approach to path length estimation, like the one presented here, is required. This is particularly true, as implicitly the paper uses the "manual method", which is far more concrete and can be automated, to validate their approach. There is likely a good reason for the signal processing approach, but the case needs to be made.

Second, the Methods provided are not sufficient to allow other researchers to adopt the procedure. It needs more detailed explanations of the decisions made, guidance for researchers who may wish to apply this method across river environments, and an in-depth discussion of the assumptions and simplifications inherent in the method (for example, I find the discussion in Lines 307-323 both too surficial and coming far too late in the paper).

Third, it does not seem obvious to me that the paper tests the stated hypothesis that path length can be inferred from changes in morphology. Some of this is a definitional issue, where the lack of clarity in the term "path length" (versus the undefined "characteristic path length") muddles the paper. In the field scenario, the paper compares the morphologic estimate of path lengths with tracer path lengths, whereas in the lab scenario, the paper compares the reach-averaged bedload transport estimated with the morphologic method against a measured bedload flux. This is a different comparison, as the morphologic method presented in Equation 3 also contains terms other than the path length (mainly the measured volume of erosion). This might not be a problem if either the true volumes of erosion (i.e., including compensation) or the displacement distances of particles (using tracers – as difficult as they are to apply in a stream table) could be more clearly constrained. I imagine the erosion volumes (including throughput) could be constrained with a full sediment budget for each experimental period, which might help with linking the morphologic signature to the transport processes (and be used to strengthen the discussion of the throughput index).

Finally, I know that this recommendation might be beyond the scope of the paper presented, but I encourage the authors to consider ways to incorporate the more complete output of the signal processing approach (i.e., more of the IMFs) into descriptions of sediment transport processes.

The signal processing approach seems like it may be a powerful way to describe the heterogeneity in bedload transport processes and encourage its further development and exploration.

Below, I highlight places that the manuscript could be improved:

Introduction:

1. This paper requires a clearer definition of path lengths. For example:
    1. The initial definition of path length (line 25) is "how far sediment travels". This wording seems to read that there is a single path length that describes the entire study segment. Later in the paper (line 322) the authors claim "in reality there is not one path length but rather a distribution." These two statements seem to conflict. Generally, I've always understood "path length" to describe the travel distance of one grain during an event (for which a population can be described through a distribution). Here, the authors seem to use the term "path length" to describe a characteristic path length. This is not a problem, but it should be explicitly stated.

Author's response- This is an important point that we have now tried to clarify. We are referring to the characteristic path length and have expanded the introduction significantly to discuss the differences in path length terminology and clarify what we are aiming to estimate. We have also included further discussion of this topic in the discussion (See lines 60-67 and 78-90 and sect. 5.1.4 Using the IMFs)

    2. Also, the reader would benefit from some discussion of the characteristic path length. How does the characteristic path length relate to a hypothetical path length distribution as measured by tracers? Is it the mean? The mode? Or is it just a value that makes the morphological method "work"? This is particularly important as the paper estimates a characteristic path length and compares the estimate against tracer displacements and bedload transport measurements.

Author's response- Absolutely! We think this is an important topic for discussion also in light of our study (See lines 5.1.3 Morphological controls and sect. 5.1.4 Using the IMFs).

2. The review of previously presented methods to estimate a characteristic path length from a DoD is unsubstantial. The authors briefly mention two methods 1) the pairing of two morphological units from Neill (1971) and 2) The pairing of zones of erosion and deposition discussed in Section 2.2 (although this subsection requires citations). Other sediment-budget based methods have been presented as well. Calle et al (2020) (DOI: 10.1002/esp.4765) and McDowell et al (2021) both present ways that sediment budgets can be used to estimate characteristic path lengths.
    1. Extension of this review is important as the manuscript does not highlight the need for their presented method. Under what conditions is the somewhat abstracted

approach presented here an advantage over the more concrete approaches that have been used previously?

Author's response- We agree that the review should be expanded and have included both Calle et al 2020 and McDowell et al 2021 as well as other studies in the introduction. (See extensive changes to the introduction). We appreciate the value of these previous approaches and are not so much presenting the signal processing method as being a superior approach but rather an alternative way to view the erosion and depositional patterns by decomposing the overall pattern into its composite parts (the IMFs). We believe that the IMFs provide the potential to expand the understanding of the heterogeneity of sediment transport and how it relates to a characteristic path length of bulk sediment transport and have tried to make that clearer in the manuscript.

3. I think the paper would benefit from improved clarity in the paragraph starting on Line 50. For example:
    1. It does not seem obvious to me that the paper tests the hypothesis that path length can be inferred from changes in morphology. It seems to be testing whether the signal processing approach can adequately represent a characteristic path length (whereas path length being inferred from morphologic changes seems to be a core assumption of the hypothesized method).

Author's response- Yes, you are correct, and thank you for bringing this to our attention. We have reworded this section to clarify our objectives and the underlying assumptions/hypotheses on which they are based. (See lines 120-128).

    2. What is an "event" in Line 51?

Author's response- We are referring to a competent flow event. We have clarified this in the manuscript (see line 121-122).

    3. The objectives should mention a signal processing approach.

Author's response- We agree and have reworded the objectives (See lines 120-128).

Methods:

Broadly, I feel that the methods section would benefit from some expansion and revision.

1. Lines 80-82 – These requirements are not explicitly checked for in this paper. In the field scenario, I am unsure how one would do this so its exclusion is fine, however, in the laboratory one, these requirements should be confirmed.

Author's response- We aimed to do this by using the time scale for morphologic evolution as the time step between DEM acquisitions (see lines 257-263 and 257-276).

2. Section 2.2 is a description of an application of paired erosion and deposition zones. This section needs citations as the method is not original. It also needs more methodological

detail. For example: How was the middle of each patch determined? The statement "use our knowledge of morphological processes to make a best estimate" should be expanded past the example provided in Line 94 and supported with citations.

Author's response- We have expanded this section to explain the basis for our reasoning and the previous work on which it was based (see lines 141-161).

3. Lines 95-97 seem to be the argument for the automated signal processing method by claiming that the "pairing" method is subjective. Would a script that automates this be similarly subjective? Erosional and depositional patches could be found with an area greater than a particular value, the center of mass of each patch could be identified, and the distance from erosional to depositional centers could be measured. Why choose a signal processing approach for automation over the one I just described? To be clear, I'm sure there are good reasons to do so, but the paper needs to clearly make that case.

Author's response- We have expanded the section on the manual method to clarify why we are using it as a comparison and its strengths and weaknesses (see lines 158-161).

4. Figure 1:
    1. Can a flow direction arrow be added?

    Author's response- Yes, we have added a flow direction arrow.

    2. Panels A and B seem superfluous.

    Author's response- Agreed. They have been removed.

    3. Based on my reading of the paper, it seems to contend that the characteristic path length estimate applies to the entire study reach? Is that the case? Does the estimated path length in Fig 1c also apply to downstream erosion/deposition pairs?

    Author's response- Yes, we have updated the figure to show additional examples of what might be the characteristic path length

5. The description of the signal processing approach in Section 2.3 needs more detail:
    1. A brief overview or methodological description of signal processing approaches in general might help researchers who are interested in the method but are not familiar with the toolset.

    Author's response- Great point. We have now expanded this section to include more information on other signal processing approaches and the VMD-HD approach in particular (see lines 198- 210).

    2. What should users consider when choosing the bin size? The bin size greatly impacts the results of signal processing approaches.

    Author's response- Definitely! We have added information to help users choose a bin size appropriate for their study by citing Calle et al. 2020 and the criteria they used (see

lines 171-178). As well as a discussion of the risks of setting too large a bin size (lines 502-503 in the discussion).

3. Similarly, how does the raster cell size impact the analysis? The method uses summed elevation change in each bin instead of volume so the cell size might matter.

Author's response- Very true. We did not experiment with raster size but we can imagine that a larger cell size would aggregate change and therefore have a similar effect as underbinning. These are decisions that need to be addressed in any change detection using DEMs. We have added this in the discussion (see lines 492-497).

4. Can you expand the reasoning for the choice of VMD over other approaches (line 122)? Expansion of the general description of VMD would be helpful for lay users (pushing the mathematical description to other citations seems appropriate).

Author's response- Yes. We have added a brief overview of the advantages of VMD and pointed readers to a quantitative review (see lines 198-242).

5. Why are there only 5 IMFs? Would this approach work with 3 or 7?

Author's response- Thank you for raising this question. This is discussed in the signal processing literature and we cite those studies and have conducted a crude sensitivity analysis using 3, 5, 8, 15, and 25 IMFs for the calculations. We see that 3 appears to be too few because the longer wavelengths are not present. Whereas, more than 5 increases the number of short wavelength IMFs but does not drastically affect the wavelengths we consider to be physically meaningful (on the range of cm to m as opposed to mm). We have included this in the manuscript (see lines 226-242) and the supplementary information (Figures A2 and A3).

6. Are wavelength and period the same thing in this section? It should just be wavelength (because there is no time domain), correct?

Author's response- Yes correct. We have clarified the language (see line 208).

7. More specificity with language would be helpful in section 2.3. What do each of the 5 IMFs specify (what is the y-axis in Figure 2c)? What are the authors computing the PDF of in line 129 (there seems to be a probability and a downstream distance, but I am unsure what the probability refers to, although I can guess)? What is the "original data vector" (Line 130)? What smoothing was applied? Is it the measured net elevation change in each bin?

Author's response- We apologize for the lack of clarity here. We have added the axis information to the figures. Yes, the original net vector is the net elevation change in each bin. We have clarified this language (see line 211). The smoothing was only applied when calculating the PDFs and it was a kernel density smoothing (line 214) after Ma et al., 2017.

8. The output path length estimate would be for the whole DoD, correct? How suitable is one path length estimate for reaches with geologic/geomorphic controls? I guess

the broader statement is there needs to be some guidance for users for evaluating whether a study extent is suitable.

Author's response- Yes, the path length is for the whole DoD. In the discussion we mention potentially segmenting the DoD when multiple channels are present (lines 549-553). We are not sure how geologic/geomorphic controls may change the results but the assumption is that if there is forced deposition from boulders, wood, etc. It would still be captured on the DoD and therefore taken into account with the method. This may be an advantage of our method over just using channel dimensions for example. However, as pointed out in the text, a proper answer to this question requires additional applications of the method in other contexts in future research.

6. Figure 2:
    1. Panel A: Add a flow direction arrow, key, colormap. What is the source of this image?

Author's response- Thank you. We have now added these items. The image is an original orthophoto and that is now stated in the description.

    2. Panel B:
        1. Left panel – the y-axis doesn't seem to be elevation change (is it elevation?). The x-axis seems to be distance from outlet, not distance downstream (this applies to all panels)

Author's response- The y axis is the sum of the elevation change in a bin. The x axis is the length of the DoD starting at 0 for the most upstream section. These have been clarified in the figure and description.

        2. Right panel – What are the units? Is it summed elevation change?

Author's response- Yes, the scale is off after the line is detrended to go through 0.

    3. Panel C: What is the y-axis?

Author's response- They are the decomposed IMFs. The values are unique for each panel and are the central frequencies that we then convert to path length. We felt this would not be useful to include in the conceptual figure.

7. Line 133 – Is the claim that erosion and deposition that does not align with longer wavelengths just noise? Is there an assumption that erosion and deposition is regularly spaced?

Author's response-Yes, we assume that the majority of erosional and depositional sites are somewhat regularly spaced. We realize that this is not always the case, especially with external forcing (i.e. large wood, boulders, manmade obstructions) but that assumption does underline the idea of using the periodicity of erosion and deposition as

a proxy for the characteristic path length. It is the assumption that the shortest wavelengths are associated with noise (i.e. IMF 1 and 2) because they correspond to very small distances relatively both for the flumes and field but as mentioned by this reviewer (comment 8), the range of IMFs may represent different aspects of the geomorphic system. We have edited the manuscript to include a specific discussion of IMF 4 and IMF 5 and what they might represent with respect to sediment transport and the physical path length distribution. See extensive changes to the manuscript (Sect. 5.1.4 Using the IMFs)

8. A broad comment - by only choosing one IMF, it seems that lots of potentially useful data are being discarded. I wonder if the smaller frequency IMFs can be used to improve descriptions of the geomorphic system.

Author's response- Yes, we suspect the same! See previous comment.

Flume and field data

1. Lines 139 through 144 – I'd make explicit that there are four flume experiments and three field sites (i.e., "three separate bars" is three separate sites).

Author's response- We have updated the manuscript (see lines 256-257 and 248-251).

2. Line 149-150 – Can you describe the planform morphologies?

Author's response- We have added this information in Table 1.

3. Was there sediment feed for the flume studies?

Author's response- Yes, we added a sentence in the description of the laboratory experiments with the details of the sediment feed (see lines 257-259).

4. Line 166-168 – Why is it important that the flume experiments have similar volumes of erosion and deposition in each run?

Author's response- We removed this sentence as it was misleading. The use of T_ex is a way to normalize time and therefore have a similar temporal evolution across different runs.

5. Section 3.2 – Include a paragraph about the San Juan River, its morphology, flooding regime, etc.

Author's response- We have added this. See lines 287-293

6. Spell out standard deviation the first time you use it (so readers know what SD means).

Author's response- Fixed. Thank you for pointing this out.

7. Line 190-192 – I understand that paired topographic and tracer data are difficult to find, so the San Juan, despite its limitations is a good choice; however, I believe that this claim needs some expansion. How much of the area is submerged? How much area do the changing

water levels impact? Can you give the readers an idea of the scale of uncertainty this limitation provides?

Author's response- We agree and have updated the text to give justification for the use of this data with the given limitations (see lines 297-310).

8. Line 211 – Include an equation for SMAPE – also explain why SMAPE was chosen over the more commonly used MAPE.

Author's response- Thank you for raising this point. After consideration, we have decided to use the relative percent error and feel this is more appropriate (see lines 333-337).

9. Line 213 – How? Which metrics are being compared?

Author's response- After deliberation, we have decided to use the San Juan data as a qualitative comparison to see how the characteristic path length compares to the physical path length distributions. We felt that the error metrics were not appropriate because we are aiming to estimate the characteristic path length, not a distribution.

10. A general note, the San Juan River has significant amounts of sand, whereas the tracers were gravels (ranging from 3-9 cm in diameter, I believe). If sand is making up a significant portion of the volumetric change, one would expect morphologic methods to overpredict transport lengths.

Author's response- Yes, great point! This is especially the case with bar 15 which had a higher proportion of sand. We have now added this information to the paper (see lines 414-416).

Results

1. Figure 3 – What is the y-axis on the shown IMF? Have you tried plotting a y-axis (with labels) on the figure?

Author's response- We have remade this figure including a y-axis.

    1. Are there manually derived distances for each patch to patch combination? How were patches with vertically adjacent erosional and depositional areas handled (in panels with Discharges 1.5 and 2, particularly)?

    Author's response- We did not measure every patch to patch combination but tried to capture the majority of the largest ones. We measured 48 lengths for each discharge. We did not match up vertically adjacent patches as sediment transport was assumed in a primarily downstream direction.

2. I had to go back and reread multiple times to figure out how all of the different experimental runs that built the data into Figures 4 and 5. It might be easier if the "Flume and field data" section explicitly mentions how many experimental runs were completed at each discharge (or if they were collected sequentially, how many DEMs were collected for each experimental run)

Author's response- Yes we agree and apologize for the lack of clarity. We have clarified this (see lines 273-276).

Discussion

1. Line 306 – how can the method select an erroneous IMF? What physical basis exists to claim a selection is erroneous?

   Author's response- We believe that the scale of the lower IMFs (1 and 2 especially) is too small to be meaningful i.e., on the scale of mm.

2. Lines 307-320 – How can more than one distance be a suitable "characteristic path length"? It seems to me that the underlying evaluation of this method is the matching of erosion and deposition zones, which the paper claims is subjective. I think that subjectivity in methods is fine and often desirable, but that undercuts the stated need for an objective method.

   Author's response- This was poorly worded and we have now tried to clarify throughout the manuscript that the decomposition is desirable to extract the underlying periodicity and not that all IMFs represent a characteristic path length but that perhaps a range can be estimated between IMF 4 and IMF 5. See sect. 5.1.4 using the IMFs.

3. Line 321-323 - In my view, a weakness of this paper is the lack of clarity in explanation around the assumptions and simplifications inherent in the method. The signal processing method seems like it contains a lot of information across the IMFs that can be used to approximate or simplify topographically derived path lengths across the entirety of the DoD. Topographically-derived path lengths have their own assumptions, which are discussed in the discussion (throughput, compensating exchange), especially when compared with tracers (where the issue of whether or not the grain sizes of the tracer particles are representative of the topographically active sediment – See McDowell et al. 2021 for a discussion - as grain size has been observed to impact path length measurements – see Hassan and Bradley 2017). Like with tracers (McDowell et al., 2020), topographically estimated path lengths have been observed to vary with location along the channel (Calle et al., 2020, McDowell et al., 2021), complicating the implications of reducing these values to a simple number.

   Author's response- We agree and have tried to clarify the assumptions and simplifications we make from the beginning and throughout the manuscript (see the extensive changes made throughout the manuscript).

4. I think the paper would be strengthened if sediment budgets were determined for the laboratory experiments. This would allow for the use of the throughput index address the questions in 5.1 and strengthen the discussion about time-windowing.

   Author's response- We computed the reach scale sediment budget for the flume experiments and report the volumes of erosion and deposition for the different time windows in Table A1. We preferred not to use the throughput index, as it cannot be easily computed in the field, as opposed to the proportion of active width. We did not want to further expand the paper analysis

5. I like the discussion of the morphologically active width and Figure 10. It's a useful (and new, at least to me) warning for those who wish to apply the morphologic method.
    1. Change the X-axis label in Figure 10.

       Author's response- Thanks, we are happy the figure is useful. We have changed this figure.