This comment is in response to anonymous reviewer 3 (RC3).
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.

This manuscript presents a signal processing approach to estimate a characteristic sediment path length from DoDs, which in turn are used to estimate sediment transport. This work builds on a previous seminal work by Vericat et al. (2017), who anticipated the potential of DoDs combined with virtual sediment velocity (~path length) estimates to enrich the potential of the 'morphological approach' and quantify sediment fluxes in gravel-bed rivers. The topic is interesting, so the paper could potentially be of interest to the river science community and fits well within the scope of E*arth Surface Dynamics*.

The manuscript is very well written and organized. However, although the manuscript and the data presented seems in general to be good (at least to me), I found some crucial weaknesses. Consequently, I believe that the draft still needs some important work before its publication. Several reasons lead me to this assessment, which I will try to explain and justify in the following paragraphs. Nevertheless, I am confident that the authors will be able to adequately address these concerns and, after major reviews, prepare a manuscript that will be closer to acceptance.

Hypothesis: I have several concerns about the main hypothesis of this paper as stated by the authors. They explicitly write that their working hypothesis is that the "*sediment moves from one area of net deposition to an area of net erosion during the time period between DEM acquisition and that this represents a characteristic path length*" (lines 85-86). However, this statement needs some preliminary terminological clarification as to how the authors define and understand the term 'path length'. In my opinion, there are (at least) two different meanings for this term. On the one hand, it could be understood as the distance travelled by individual particles during a transport event; this is the meaning typically used in tracer studies. On the other hand, 'path length' could be understood as the average distance travelled by sediment during typical channel-forming flows. This is the meaning sometimes used in studies employing the morphological approach, which is typically equated with a characteristic morphological length scale of channel (meander length, bar-pool length, bar spacing, etc).

Author's response- Thank you for pointing this out and we agree that this was not clear in the first version of the manuscript. To rectify this, we have extensively expanded the introduction to expand on the path length terminology. Further, we have reworded our objectives and clarified the underlying assumptions (see lines 120-128 and extensive changes to the introduction).

Tracer studies have long shown that path lengths during transport episodes are not defined by a unique single value as assumed sometimes with the morphological approach, but by a distribution of values, whose exact shape (gamma, exponential, heavy vs thin-tailed…) is a widely discussed topic (with hundreds of pages of previous literature). Bearing all this in mind, it seems that by 'path length' the authors are somehow referring to the 'mean' or 'average' distance travelled by bedload particles during the sediment transport episodes studied. In this respect, the real hypothesis could be stated as: 'the distance between successive deposition and erosion patches, measured in a DoD, could be used as a proxy for the average travel distance (~path length) of the bed load'. If formulated in this way, the paper would provide continuity with previous research highlighting some kind of morphological control on average bedload travel distances and would potentially enrich the debate on the topic.

Author's response- Absolutely! We have expanded the introduction to discuss the idea of "path length" vs "step length" vs "characteristic path length" the latter of which is what we are trying to estimate. We have also updated the objectives to explain why we compare our estimates of a characteristic path length to the measured path length distributions in the field and how this gives support to the idea of morphological control on average bedload travel distances. Additionally, we bring this topic up in the discussion with regard to the tracer distributions.

Apart from this, and although I appreciate the effort made by the authors in explicitly stating their hypothesis, in my opinion the manuscript does not clearly show how this hypothesis is tested. As a result, the reader cannot be sure or convinced if this hypothesis is adequately supported (or not) by the results. Then, I will encourage the authors to make more effort to link their methods to their hypothesis and to explicitly discuss the results in the light of the hypothesis.

Author's response- We agree that the first version of the manuscript was not clear and have reworded the objectives and their underlying assumptions (see lines 120-128).

Introduction: The review of previous literature presented in the introduction needs to be expanded, as I missed some recent works that are clearly relevant for this work. For instance, Bakker et al. (2019) (https://doi.org/10.1029/2018JF004811) presented a nice application of the morphological approach, based on DoD, where they presented a spatially distributed characterization of bedload in a braided river. I think this paper deserves (at least) a mention in the introduction. Then, there is the work done by Calle et al. (2020) (https://doi.org/10.1002/esp.4765), which is even more relevant because these authors present a workflow for estimating travel distances from DoD, which should be commented and discussed. What are the main novelties of the present manuscript compared to Calle et al. (2020)?

Author's response- We agree that these are important studies to include and have expanded the introduction to include these articles and much more literature on applications of the morphological method. We have also tried to highlight the utility of our approach compared to the previous applications both in the introduction and discussion. (See extensive changes to the introduction and discussion).

Materials and Methods: There are several aspects of the methods used in this paper that need clarification. In the absence of this information, it is difficult to assess the work of the authors.

For instance, in lines105-108, the authors comment that the DoD is divided "*into a series of equally sized bins*". However, there is no comment on the exact size chosen for these 'bins'. In addition, some sensitivity analysis of the effect of bin size on the final results would be welcomed.

Author's response- We agree and have expanded the methods section to discuss bin size selection (see lines 171-178 and lines 600-605 in the discussion).

Regarding their flume experiments, in line 146-147 they state that the flume was filled "*with sand characterized by a median diameter (D50) of 1 mm*". However, they do not specify whether this sediment is well- or poorly-sorted. Do you have any idea?

Author's response- The sand was homogenously 1mm. We have clarified this (see lines 253-254).

In addition, I have some concerns with the use of San Juan river data for the present work, as long as in this set of data, areas within the channel that were inundated in both the old and new DEMs were not used, thereby limiting the DoD analysis to areas above-water (MacQueen et al., 2021). Indeed, MacQueen et al. (2021) explicitly said: "*[DEM] were not used to calculate complete reach-scale sediment budgets due to the lack of in-channel topographic data and stage differences during each LiDAR survey affecting the relative portion of the river bed that was exposed*". This is a major limitation of the present manuscript. The authors should provide information on what proportion of the San Juan channel is represented by underwater areas and excluded from the DoDs. They also need to provide a better justification or discussion on how they believe this does not bias their results, and not simply reduce it to what they state in lines 190-192. They need to better justify lines 190-192 and explain why they believe that they can use San Juan data in a way that it was prevented by the original authors who collected this data (MacQueen et al., 2021).

Author's response- We agree that this was a large oversight in the previous version of the manuscript and have now included information on the submerged area of the DoDs and have also explained why we believe this data is still relevant for our purposes (see lines 299-311).

Results: In general, I think the presentation of results needs to be improved.

First, flume and field data are not treated in the same way, making it difficult to assess how their results do or not validate their hypothesis. I understand that this could be due to the different nature of the source data, but some effort should be done to homogenise the presentation of results. For example, the path lengths obtained from flume DoDs are validated against "*manually measured distances*" (line 231). Why is this not also done for the San Juan DoDs? By the way, there is nothing in the methods section (or anywhere else in the manuscript) about how this manual measurement of path lengths was done. This should be explained in at least a few lines, so readers could properly evaluate their work.

Author's response- We agree that this was an issue as the original manuscript was written. We have now clarified that the San Juan River data is used to see how the characteristic path length of sediment transport might compare to a physical path length distribution. (See lines 135-137). We have expanded the section on the manual method (see lines 141-161).

In addition, the presentation of results for the San Juan river is really poor (just one paragraph). To my knowledge, MacQueen et al. (2021) presented results from three or four tracer surveys in the San Juan River: 2016-2017, 2017-2018 and 2018-2019. Not much information is provided in section 4.2, so we don't know if all the numbers presented in this section refer to the average of all these tracer surveys, to just one survey… However, in figure 7 it seems that the authors are using just one survey. Why is this?

Author's response- We agree that the previous result section for the San Juan River data was very sparse and has now been expanded (see Sect. 4.2) however because this was more of a qualitative comparison, we use the discussion to explore the idea of how a characteristic path

length compares to the distributions (see lines 455-477). We apologize for the lack of clarity in the previous version of the manuscript and have now clarified that there were only DoDs corresponding to the tracer deployment for one year, 2018-2019. The other DoD spanned multiple tracer deployments and therefore we not desirable for our purposes (see lines 248-251).

Discussion: Again, I have some concerns about how the authors discussed their results.

In lines 284-291, the authors attempt to discuss the fact that their path length estimates do not change with flow discharge. This seems counterintuitive, as we would expect average travel distances to increase with flow intensity, as many decades of previous tracer research have already shown. The authors managed to put forward two possible explanations, but I am not entirely satisfied with how they present and discuss them.

The first proposed explanation is that at higher discharges the characteristic path length is greater than the average distance between erosion and deposition patches (which I think is probably the correct explanation). But in fact this is not an explanation, but a bias in their method, which results from their assumption that the sediment simply moves from a deposition patch to the closer erosion patch downstream. However, sediment can travel (and often does) further than a morphological unit in gravel-bed rivers. Indeed, there is some field support for this in previous tracer data. Vázquez-Tarrío et al. (2019) conducted a meta-analyse of previously published tracer data, in which they compare the mean travel distances of sediment to the morphological length scale of the channel. They observed that at high discharges, mean travel distances of tracers can be larger than one morphological unit. The same was observed by Liebault et al. (2012) in a wandering river. In this respect, some discussion could be done on how to exploit tracer data to better constrain how the average number of morphological units travelled by bedload particles increases with discharge. This could be a clue on how to improve in the future their method.

Author's response- Yes, these are great points! We have expanded the discussion to engage with these ideas (see sect 5.1.1 Flow effects).

In this respect, the approach presented by Calle et al. (2020) seems to be more appropriate to deal with these situations than the one presented in the present manuscript. Calle et al. (2020) attempt to account for this increase in travel distances with flow discharge by using the balances between the volumes of erosion and deposition. Consequently, this previous work of Calle et al. (2020) should be discussed here. However, it is not mentioned in the whole manuscript.

Author's response- We recognize the importance of including the work of Calle et al. (2020). and have included references to their study. We have also tried to emphasize that our study is focused on a different objective (the characteristic path length as represented by the periodicity of erosion and deposition).

Another issue follows from this fact that their method is insensitive to flow discharge. Indeed, the average distance between erosion and deposition patches should scale with channel dimensions, i.e., in principle, we could expect larger distances between erosion and deposition sites in larger channels and smaller distances in smaller channels. Then, if the method is insensitive to flow discharge, we might ask what the advantage of such a sophisticated approach is, compared to the more straightforward procedure of simply considering the channel width (as Beechie, 2001) or the channel width times 5-7 (typical values for average pool spacing in gravel bed rivers) as the characteristic path length. Some discussion of this should then be included. In doing so, the authors will potentially provide interesting information on three questions. First, is their signal processing method more efficient or not than simply using the channel dimensions as a proxy for path length? This is an interesting result in itself, as it could help to simplify future works. Second, is there a consistent relationship between the distance of erosion-sediment patches observed in the DoDs and the channel dimensions (width, pool-pool spacing, etc)? Finally, third, is this scaling consistent between flume and field data?

Author's response- Thank you for raising these questions! We have conducted additional analysis and included the results which we believe strengthen the manuscript. Additionally, we have expanded the discussion to include these concepts (see sects. 5.1.1, 5.1.3, and appendix figures A6 and A7).

Finally, Redolfi (in Vericat et al., 2017) presents an alternative for estimating the path length, which consists in using the length scale of the long axis of erosion patches in DoDs as a proxy for the average path length of sediment. Some discussion should be done in this regard and authors should explain why they believe their approach is (or not) more adequate than the one proposed by Redolfi.

Author's response- We agree that this was a major oversight of the original version of the manuscript and thank you for brining it to our attention. We have now included the work done by Redolfi (2014) and Vericat et al., (2017) in the introduction (see lines 103-109) and have made extensive changes to the manuscript in hopes of explaining why we believe that our method can contribute and build on the work done by Redolfi, Vericat et al., and others by providing a new way to think about the periodicity of erosion and deposition.