I feel that the authors have carefully considered all the points I made in my comments. I am very grateful for the cooperative attitude of the authors and therefore recommend publication of the paper, subject to some minor changes.

I must say that I appreciate the work done by the authors in reviewing their manuscript in the light of all the reviewers' comments; not only mine, but also those of the other two colleagues who also reviewed the manuscript.

However, my main remaining concern with the manuscript relates to the way in which the authors justify the importance of their work in the introduction. In order to emphasise the interest and importance of their approach, the authors somehow underestimate the value of tracer research. For instance, in lines 79-82 authors said that "*is unclear if the mean path length as measured by tracers is the best representation of a 'characteristic' path length to estimate bedload transport*" and they go on to say that the morphological approach could be used to infer a characteristic path length from changes in morphology as an alternative to tracers (lines 91-93). In my opinion the two methods are not antagonistic but complementary. In fact, tracers are the only method that provides real information about particle displacements. Inferences made by the morphological approach are surrogate estimates based on assumptions and hypotheses that may be questionable in some circumstances. Although I recognise the value of the morphological approach, that concern should not be forgotten and therefore I believe that both methods are complementary. Although my comment does not preclude my recommendation for acceptance of this manuscript, I would be grateful if the authors would consider it and perhaps rephrase some parts of the introduction.

Apart from that, I have a few minor comments. Before I list them, I would like to congratulate the authors on their work.

Minor comments

- Line 67: I think a comma is missing between "(Hoey, 1992; McLean and Church, 1999)" and "measurement uncertainty".

- Line 88: I think eq.1 deserves some reference. Maybe Ashmore and Church (1998) or Church (2006) should be cited.

- Lines 49-59 and eq. 2: The virtual velocity approach not only needs an estimation of path lengths, but also some estimation of the average active layer thickness (d_s in eq. 2). Estimation of this thickness is also tricky. Tracers and scour chains have been used to measure it in the field, whereas in the morphological approach it is estimated using DoDs. I think some mention and references would be welcomed. For instance, Church and Haschenburger (https://doi.org/10.1002/2016WR019675), Leduc and Ashmore (https://doi.org/10.1002/2017WR022438), Vázquez Tarrio et al. 2020 (https://doi.org/10.1002/esp.5027) , etc.

- Line 80-82: This long sentence is difficult to understand, at least for non-native English speakers. Please, consider modifying it.

- Line 82-84: Same remark as above.

- Lines 84-90: Here authors state that "*at formative discharges, particle path length distributions often exhibit primary or secondary modes corresponding to the location of bars, where deposition occurs*", so "*the characteristic path length, i.e., the most representative and sound value to be used in sediment transport estimations, might be*

better described by these primary or secondary modes in channels with bar morphology at channel forming flows".

I agree with this comment. But the authors seem to use these statements as an argument against the virtual velocity approach based on mean travel distances derived from tracer data (eq. 2) and in favour of the morphological approach (eq. 3). However, in my opinion, both methods suffer from the same problem. The morphological approach used by the authors estimates the characteristic path length as the distance between an erosion patch and a subsequent deposition patch. This neglects the possibility that particles can travel further from a single morphological unit, so it also implies that secondary modes of particle displacement are not considered. This relates to my previous comment and I sincerely believe that the authors should consider rephrasing some sentences.

- Lines 141-142: This brings us back to my same recurring concern. To me, this hypothesis contradicts your statements in lines 84-90. If there are secondary (and more) modes of particle displacement that bias tracer studies... how can you be sure that these secondary modes do not also bias your method? As long as this method assumes that sediment does not travel further than the distance between one erosion-deposition couplet...this seems to contradict your literature review in the introduction, doesn't it?

- Lines 305-306: This may deserve further justification. Firstly, what percentage of the area is covered by the underwater channel? We need this information to be sure that the underwater channel does not represent a large percentage of the surveyed area in the San Juan River.

  Then I think your statement (that the underwater channel may not distort the patterns of erosion/deposition) would only be true if the underwater channel only acts as a sediment conveyor belt. However, if this is not the case, even if it is only a small part of the area studied, then the overall patterns of erosion/deposition in the underwater channel could well confound the path length estimates. For example, we could imagine that the underwater channel could be preferentially eroded at channel confluences/flow convergence areas, or show persistent deposition at channel diffluences or secondary flow areas.

- Lines 425-427: Again, this seems for me somehow contradictory with lines 84-90 and the idea that there are secondary and larger modes of particle displacements.

- Line 455-456: Be careful with this. What is the tracer recovery? Where are the missing tracers? Recovery rates reported by McQueen et al. (2019) in San Juan river are relatively high: around 70-75%. However, if the missing 25-30% were preferentially tracer frontrunners, then this statement could be biased.

- Line 474-477: This is a nice support to the idea that path lengths may probably increase with discharge.

- Section of Confinement: I personally like the originality of this addition to the discussion.

- Line 509-521: I appreciate that the authors have considered my previous suggestions.