

Response to referee report (version 2)-

**We would like to thank you very much for taking more time to review our article and greatly value your contribution. We believe that we have addressed each of your concerns below.**

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.

**Author's response- We agree that the two methods should be complementary, especially now in the development of this new method. We meant to say that perhaps the mode, or secondary mode, of the path length distribution is more meaningful to estimate sediment transport. So it is not a criticism of tracer studies, just that perhaps the *mean* is not always the ideal value. The identification of these secondary modes which correspond to the location of bars would be impossible without these tracer studies so we certainly do not want to downplay their importance. We have now re-worded these lines for clarity. (see lines 82-86)**

**Additionally to your point, we think that perhaps by using tracers and our method in situations with varying discharge, we can identify a threshold for when the majority of particles are traveling further than one morphological unit, and multiples of the path length estimated by the method are needed. For example the 2 l/s discharge (see discussion sect 5.1.1).**

Minor comments -

Line 67: I think a comma is missing between “(Hoey, 1992; McLean and Church, 1999)” and “measurement uncertainty”. (\*\*referring to the tracked changes doc)

**Author's response- This has now been fixed (see line 23).**

- Line 88: I think eq.1 deserves some reference. Maybe Ashmore and Church (1998) or Church (2006) should be cited. (\*\*referring to the tracked changes doc)

**Author's response- We agree and have added these references (see line 44).**

- 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 Tarrío et al. 2020  
(<https://doi.org/10.1002/esp.5027>) , etc.

**Author's response- We agree this was an oversight and now have included a line to describe this term with citations. (see lines 55-56).**

- Line 80-82: This long sentence is difficult to understand, at least for non-native English speakers. Please, consider modifying it. (\*\*referring to the tracked changes doc)

**Author's response- Thank you for pointing this out, we agree and have re-worded this sentence (see lines 31-33).**

- Line 82-84: Same remark as above. (\*\*referring to the tracked changes doc)

**Author's response- See lines (34-37)**

- 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.

**Author's response- We think this comes from the lack of clarity in the introduction. Hopefully, by changing lines (82-86) we have shown that it was not meant as an argument against the virtual velocity approach using tracers, just that the mean value is perhaps not the most representative. By rewording the introduction, we have tried to emphasize that tracer studies are fundamental in creating the relationship between path length and morphology. We have tried to clarify this in the introduction (see lines 82-87)**

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?

**Author's response- Thank you for bringing this to our attention. We were not clear in the introduction. It was not meant to be argued that these secondary modes bias tracer studies, but that these tracer studies have shown the importance of bars as depositional sites as evidenced by the secondary modes. Using tracers to estimate the virtual velocity is absolutely acceptable in our opinion and the mode or secondary mode could be considered as the path length (see lines 82-87). We have also seen that in the McQueen data, our method does pick up on these secondary modes, so it is not necessarily a bias in our method. What is a major fault**

**in our method is the inability to pick up on multiples of the bar spacing. Tracers could and should help us in the future define the conditions under which sediment is traveling multiple morphological units and potentially define a threshold for using multiples of the characteristic path length to estimate sediment transport (see discussion lines 471-480).**

- 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.

**Author's response- We agree that the percentage of submerged channel should be included and have updated this section to include that information and also tried to explain that this could bias the results but given the limited available datasets containing quality DoDs and RFID tracer data, we think it is an appropriate first application (see lines 305-315). We are currently working on a study with DoDs that includes the submerged area and hope to have more chances to apply the method in the future.**

- 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.

**Author's response- We hope that clarifying what we meant in the introduction will address this concern (lines 82-86). What we mean here is that the secondary modes from Pyrcce and Ashmore's study were often the first bar downstream of the insertion location. So the first mode was generally the tracers that moved very little from the point of insertion and the secondary mode was the first depositional unit downstream. This is what we mean by the next depositional unit downstream.**

- 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.

**Author's response- Good point! We have included this caveat. (see lines 462-465).**

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

**Author's response- Yes, we agree and hope to conduct further studies to verify this hypothesis.**

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

**Author's response- Thank you!**

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

**Author's response- Thank you for your valuable suggestions. They greatly improved the quality and completeness of the article.**