General comments

“I think the conclusions regarding dune tracking are more specific to how the method was applied in this study and to this field site. Thus, I don’t think that general conclusions about the accuracy or utility of that method can be claimed.”

You are right, DTM results are more specific to the study site especially to the dune scale that can vary with the presence of bars (Le Guern et al., 2019a). The application of the DTM was employed to have a second method (usually used in sandy-gravel bed rivers) to compare with aDcp and hydrophone measurements. Results would be improved if the study was centered around this method but to compare all these methods it is very time consuming in the field and protocols have to be adapted.

“The authors state that acoustical power was not affected by water depth, but no data are presented to demonstrate that conclusion. Because acoustical power is expected to be affected by water depth. That should be demonstrated, rather than asserted.”

Please see Specific comment SC12.

Specific comments (SC)

SC1

"L. 52-54: Poor sentence construction. Rewrite. Likely needs two sentences."

"*In this work, we compare the efficiency of active and passive acoustic techniques to quantify bedload transport. The investigation took place in a reach of the Loire River (France) characterized by the presence of migrating bars and superimposed dunes (Le Guern et al., 2019b)."*

SC2

"L. 57: What are "bedload axes"? Need to define or use more descriptive term."

We agree that this term was unclear. We modified the sentence by: "*to estimate the accuracy of acoustic methods to measure cross sectional variations of bedload fluxes for various discharge conditions*".

SC3

"L. 58-59: Not clear how this is different from #1"

The third point aims at exploring bedload variations associated to bedforms with an aDcp and a hydrophone. Are they sufficiently accurate to record these bedload variations? The first point is related to the calibration of several acoustic methods with a direct measurement method without linking bedload to the presence of bedforms. We modified this sentence in order to be clearer: "*3) to investigate the capabilities of hydrophones and aDcps for capturing bedload variations along bedforms.*"

SC4

"L. 76: Why "theoretical" hydrophones?"

This is theoretical position of hydrophone drifts because when we drift we were not able to follow exactly these theoretical lines. However, we agree that this term can be confusing. We decided to delete it.

SC5

"L. 88: need full description of sampler (dimensions, weight, bag size, mesh size, etc.). Or reference to where that description can be found."

"*Samplers consists of a sampling basket mounted on a frame. The sampling basket have a rectangular mouth of 0.05 m high and 0.085 m wide. Complete description of the sample can be found in de Vries (1979)*."

SC6

“L. 94: This calibration factor seems to imply that the sampler is not actually "isokinetic". If it were truly isokinetic, it should not need a twofold correction factor.”

“L. 97: This is an important reference for sampler calibration, but it's a "lecture note" that does not appear to be publicly available. If this reference is not available, these sampler details (calibration method and values) need to be described in the paper or an appendix.”

The reference is a textbook. We added the DOI in the reference list. Details of the calibration procedure are in two Dutch reports that we cannot find (Delft Hydraulics Laboratory, 1958 and 1969). But, additional elements are described in de Vries (1979). The first calibration tests consisted to compare the BTMA bedload catches with the average flume sediment transport. In a second time, tests were made by weighing catches in a sediment trap of the same size than sampler mouth. Sediment mixtures used during these tests was coarser than bedload of the Loire River with D_{50} varying between 2.5 mm and 6.4 mm. Both test series concluded to the same calibration coefficient of 2 (efficiency of 50%). This means that actual transport is two times higher than those measured with the sampler. The average efficiency of a basket sampler as the BTMA sampler is about 45% (Hubbell, 1964). These tests established a calibration curve linking unit bedload transport rates with caught volumes of sediments. It is mentioned in Boiten (2003) that this calibration factor was not including the possible losses of sediments finer than 0.3 mm (size of the mesh). The sand loss was estimated during flume experiments with a similar samples by Banhold et al. (2016) to 50% in average but with a mesh size of 1.4 mm and varied sediment mixture (D_{50}=[0.8-10] mm). There is no detail about the hydraulic coefficient of the BTMA which is the ratio between velocity in the sampler and flow velocity in de Vries (1979). But, author argued that the BTMA construction promote the transport coefficient at the expense of hydraulic coefficient. In comparison, the Helley-Smith sampler tend to overestimate bedload with the ratio between the flume sediment transport and the sampler sediment transport that vary with flow velocity from 1.2 to 2.6 (Helley and Smith, 1971). In the new version of the manuscript we present the BTMA as a pressure differential sampler.

“*Suggested values of α and b were adopted from Boiten (2003) which mentioned that the trap efficiency factor not include the possible losses of sediment finer than 0.3 mm (mesh size opening). This calibration factor come from successive calibration procedures concluding to the same calibration coefficient of 2 (de Vries, 1979).*”

SC8

“L. 106: Which approaches are considered the "empirical" and "calibrated"?

“I think everything described below is essentially empirical, but I'm not sure which approaches are calibrated?”

The calibration approach is the approach which attempt to correlate bedload transport rates calculated from BTMA measurements with apparent bedload velocity measured by aDcp (Rennie et al., 2017).

SC9

“L. 113-114: If the boat was static, wouldn't it be better to assume zero boat velocity than to assume the boat velocity equaled the bed (bedform) velocity?”

The boat was almost static in the GPS referential (except some negligible lateral movements) but in the bottom track referential was mobile, due to mobile bed. When GPS data were missing, we considered the boat static in the GPS referential and we computed apparent bedload velocity directly from boat displacement in the bottom track referential.

SC10

“L. 127-128: There is no ds in equation 4. Is there a typo in the equation, or in the sentence? It seems like there should be d_s *in the equation. If there is a constant ds, what is it?”*

You are right, this was not clear. According to other referee comment, we changed the part of the method by separating the 2 kinematic models and explaining that the first one assumes that maximum bedload thickness is a single particle:

“$q_sADCP=\frac{4}{3}\,\rho_s\, r\, V_{a\,proj}\times 10^{3}$; (4)

Where $r = D_{50}/2$ *is the particle radius,* D_{50} *is the median sediment diameter (m),* ρ_s *is the sediment density (2650* $kg.m^{-3}$*). In this model, it is assumed the maximum bedload thickness is a single particle.*

$q_sADCP= V_{a\,proj}\, d_s\, c_b\, \rho_s$; (5)

Where c_b *is the concentration of the active transport layer considered as the saltation height (van Rijn, 1984), and the van Rijn (1984) formulation was adopted to compute the active layer thickness (*d_s*) as a function of the hydraulic condition and sediment grain size:*”

SC11

“L. 159-160: This is the only mention of interval time and it's not clear how many repeat profiles were measured to measure bedform celerity?”

Bedform celerity was estimated from two repeated profiles for each sampling point. You can find mean interval time for each survey in the appendix D.

SC12

“L. 187-188: As this is a new method, this should be explained in more detail. Especially because acoustic power is affected by transmission losses that are a function of distance. It may be that the losses are negligible in the range considered here, but that should be demonstrated rather than just asserted.”

Several tests were carried out to ensure that these acoustic power variations are not due to the variation of the distance between the hydrophone and the riverbed. A total of 20 drifts were performed with two hydrophones on the same horizontal location. The vertical location was constant for hydrophone P1 (0.4 m depth) and variable for hydrophone P2 (from 0.4 m to 1.6 m depth). 3 drifts were done for each P1 and P2 configurations (except for P1 and P2 at the same depth, 2 drifts). When P1 and P2 are at the same depth, the acoustic power ratio is about 1.06, so the two hydrophones measured approximately the same acoustic power. This ratio doesn't evolve linearly with the proximity of river bed (depth ratio increasing). Even if the largest acoustic power ratio is observed for the largest depth ratio, it varies between 0.98 and 1.15 with a mean standard deviation of each configuration of 0.03 (see figure 1). This means that acoustic power can be considered as independent of the listening depth until 85% of the water depth.

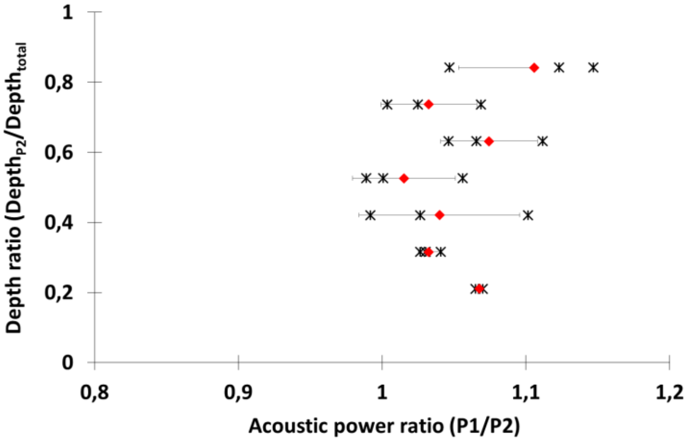

Figure 1: Variable vertical position of hydrophone in the water column and associated acoustic power variation (red dots are the mean values of acoustic power).

SC13

"L. 238: This should be in terms of qs as the dependent variable."

We presented the equation with the acoustic power as the dependent variable to make the comparison with Geay et al. (2020) easier. This did not influence the result of the regression because we applied a Reduced Major Axis (RMA) regression. Effectively, a classical regression aim to examining the dependent variable (Y axis) and the deviation of Y values from the fitted line are minimized. The RMA minimizes the deviations of the observations from the fitted line in both X and Y directions (Davis, 2003).

SC14

"L.307-308: While I think it's valid to use the hydrophone to look at the spatial distribution as done above, I don't think the direct comparison with the BTMA measurements is useful, because the hydrophone is calibrated to the BTMA. Some will "overestimate" and "underestimate" by definition of the calibration."

Here, we considered that our calibration equation was verified and we compared results of this equation with BTMA measurements. Of course, the overestimation or underestimation depends of the calibration equation but we believe that it is interesting to see when the calibration could differ from BTMA sampling.

SC15

"L. 346-347: Recently, Leary and Buscombe (2020), Ashley et al. (2020) and probably others have argued that repeat surveys with multibeam sonar are suitable for accurate measurements."

Here we wanted to highlight that the samplers were the only direct measurement of bedload in the field that are used as a reference measurement. We propose to reformulate this sentence: "*Despite their lack of accuracy and their low spatial representativeness, isokinetic samplers allow a direct measurement of bedload and represents the only reference measurement of bedload in the field*."

We agree that the multibeam echosounders are helpful for the measurements mentioned here. The main problem is due to the geometry of the Loire at this site (wide and shadow) that makes the multibeam echosounding pretty difficult to perform. This was already done on the Loire (Claude et al., 2014; Wintenberger et al., 2015) but on study sites that were smaller.

SC16

"L. 371-373: The idea of a "global" calibration" for bedload by acoustic power is intriguing, but I have not yet had a chance to read the referenced paper in detail (it was only published this year). Given the wide variation in conditions that I think most would expect to work against a global calibration, a bit more discussion about why this might be possible would be helpful."

The question of the possible existence of a global calibration curve was, as you mentioned, addressed in Geay et al. (2020). It could be possible because the intensity of acoustic power is proportional to the number of particle impacts, the size of these particles and the impact velocity. Theoretically, if we consider two different sediment samples (one finer than the other) that produce the same acoustic power and the same bedload rate, the finer one produce more noise at high frequencies and less noise at low frequencies. This is the opposite for the coarser sample. The high frequency noise produced by the finer sample is compensated by the low frequency noise of the coarser sample. Moreover, for the same bedload rates, the fine sample is composed by more particles than the coarser but can produce the same energy as energy is related to the number of impact and the size of sediment. These assumptions do not take into account the problem of acoustic wave propagation (Geay et al., 2019).

SC17

"L. 406: Not clear what exactly is meant by this? Instantaneous?"

Here "*punctual*" means local measurement. In other words, the DTM measure bedload over a longitudinal distance with potentially different hydraulic and sediment transport conditions whereas BTMA measure bedload at a sampling point that reflect local conditions.

SC18

“L. 408: This is highly dependent on how the method is executed. If dune tracking is done at higher temporal frequency, it can provide”

Yes, by extending the temporal frequency of DTM method, the registered bedload variation could be at the same temporal scale for both methods. But, the special scale will always differ.

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