

Dear *Seismica* Editors:

We are pleased to submit a revised version of our manuscript entitled “A River on Fiber: High Resolution Fluvial Monitoring with Distributed Acoustic Sensing,” which we are resubmitting for consideration in *Seismica*.

In the revised manuscript, we have incorporated all reviewer feedback and made substantial improvements to clarity, structure, and contextualization of our results. We have (1) condensed and reorganized Section 5.4, moving quantitative details to the Supplement; (2) expanded discussion throughout to connect our observations more explicitly to recent seismo-acoustic studies, and to established research on turbulence and supercritical flow dynamics; (3) clarified terminology, definitions, and methodological details throughout, and improved consistency between text and figures; (4) added a new figure to illustrate trade-offs in the grid search for wave velocity and source distance (Figure 4b); (5) refined figure color scales and added several supplemental figures for improved interpretability of results; and (6) clarified analytical methods and modeling rationale in Sections 4 and 5, with supporting details moved to the Supplement. Collectively, these revisions enhance the manuscript’s accessibility, strengthen its connection to recent literature, and streamline its presentation while maintaining focus on the paper’s central contributions.

Our detailed **responses to reviewer comments** are provided below, outlining how each comment has been addressed. We also include both a **clean version** of the revised manuscript and a **tracked-changes pdf version** highlighting all edits relative to the previous submission.

We believe these revisions have improved the clarity, rigor, and accessibility of the paper, and we thank the reviewers for their thoughtful input. We hope the manuscript is now suitable for publication in *Seismica*.

Thank you for your consideration. We look forward to your response.

Sincerely,

Danica Roth, on behalf of the authors
Cooperative Institute for Research in Environmental Sciences (CIRES)
University of Colorado Boulder

General review

The manuscript by Roth et al. presents the first field deployment of a Distributed Acoustic Sensing (DAS) cable directly into a flowing river environment, an important technical and methodological milestone. The authors analyze data collected along a 160-meter cable span over a turbulent channel and show how spectral variations in space and frequency reflect key morphological features, such as boulders, riffles, and pools. This is the first study to interpret DAS observations in such a setting, marking a significant advance in both environmental seismology and fluvial monitoring.

Although the monitoring window was relatively short (15 minutes), the authors justify this through the stability of the hydraulic conditions and the high spatial and temporal resolution of the data, which allow for clear interpretation of bed–cable–flow interactions. Furthermore, because no bedload transport was observed during the experiment, the authors are able to isolate and assess spectral features primarily linked to turbulence and mechanical cable responses.

Beyond the technical innovation, this work offers meaningful insight for two communities: (1) those developing DAS for environmental sensing, who will benefit from the authors' discussion of cable configuration and coupling strategies; and (2) researchers in fluvial seismology and geomorphology, for whom the results demonstrate a new way to investigate spatial variability in high-frequency processes like turbulence and (potentially) sediment transport.

Overall, I find this a valuable and pioneering contribution. The manuscript is well-organized and clearly written, and I support its publication in *Seismica*. I recommend some minor revisions to improve clarity, alignment between text and figures, and consistency in terminology, primarily to make the paper more accessible to a broad, interdisciplinary audience.

Sincerely,
Ron Nativ
ron.nativ@univ-rennes.fr

1. The title, "*A River on Fiber: Spatially Continuous Fluvial Monitoring with Distributed Acoustic Sensing*," is engaging, but the phrase "spatially continuous" may be somewhat ambiguous to readers unfamiliar with the specific context. Consider clarifying or rephrasing this term to more directly reflect the central contribution or novelty of the work, especially in terms of how the monitoring approach differs from existing methods.

We have changed the title from "spatially continuous" to "high resolution."

2. Section 5.4: I find the discussion in this section to be somewhat overwhelming. While I agree that theoretical and qualitative exploration is valuable and can help lay the groundwork for future investigations, the manuscript is already quite lengthy and analytically well-developed. I'm not fully convinced that this section adds significant value in its current form, and I'm concerned it may draw attention away from the core contributions presented earlier. You might consider moving this section, or at least a substantial portion of it, to the Supplementary Information, where interested readers can still engage with the material without it affecting the focus and flow of the main text.

We have shortened and condensed section 5.4 by almost 3 pages, and moved the details of more quantitative examples to the supplement.

3. Please consider the detailed comments below as suggestions for improving clarity, flow, and

overall readability of the manuscript.

Responses to all detailed comments are provided below.

Comments on text

36-37: “a knocking signal characterized by quasiperiodic pulses.”. This is somewhat unclear. I don’t think it can be understood without other context. Maybe better to rephrase, or explain briefly.

We have rephrased this statement for clarity.

61-62: The terms “in-stream” and “along-bank” may carry specific connotations, “in-stream” is often associated with hydrophones, while “along-bank” tends to be linked with seismic methods. Consider rephrasing this sentence to reflect that distinction, especially given Seismica’s seismological audience.

We appreciate the reviewer’s clarification regarding the connotations of “in-stream” and “along-bank,” and we intentionally chose these terms to reflect the distinctions noted. Specifically, “in-stream” refers to hydrophone (and geophone) placement, as clarified in the following paragraph, while “along-bank” corresponds to seismic instrumentation. Since this aligns with the reviewer’s interpretation, we have retained the original phrasing. However, we have revised “methods” to “instrumentation” to more clearly convey our intended meaning, as it better reflects the placement of in-stream and along-bank equipment before readers encounter the sections specifying hydrophones or seismometers.

75-76: The phrase “the inability” may be misleading. Both empirical and theoretical studies have demonstrated relationships between seismic characteristics and surface phenomena, even if those relationships come with limitations. I suggest rephrasing this statement to acknowledge that such connections do exist, albeit with some constraints.

We have updated the text to clarify this point.

78-80: You might consider citing the work Peter Thorne (e.g., [https://doi.org/10.1016/S0278-4343\(01\)00101-7](https://doi.org/10.1016/S0278-4343(01)00101-7)), as he has contributed important early insights into sediment transport using hydrophones.

We have added a citation for Thorne and Hanes (2002) and appreciate the extra reference.

80: misleading. The high-frequencies do not attenuate rapidly, but the amplitudes at these frequencies are.

We have rephrased to clarify that amplitudes attenuate at high frequencies.

85-86: and attenuation as well (<https://doi.org/10.5194/esurf-7-537-2019>).

We have noted this and added a reference to Geay et al., 2019.

100: what does “observational solution” means?

This phrasing has been removed and replaced with “practical approach for overcoming” for clarity.

109-110: footprint for hydrophones versus DAS – why is that?

DAS cables have smaller physical diameters (<1 cm) compared to hydrophones (typically 1.5 to several centimeters). The text has been updated to clarify this.

130-131: I don't fully understand this sentence. What does "previous observations" mean?

We have added several references to this claim to clarify that we refer to previously published observational studies. We discuss these studies and their relevant observations in section 5.1.

149: why "reinforced"? **and** 150: Please add some brief information on the cemented rapids, because understanding why they are placed there would give more intuition about the section's hydrodynamics nature.

We have added text to clarify that these rapids are used for recreation during high flow. The cementation is not expected to alter river hydrodynamics over the observational period, but is relevant primarily due to the resulting inability to bury the cable along the bank in these sections (mentioned in section 3.1).

152: "thick" – do you mean the depth from the sediment surface to bedrock?

This is correct. We have clarified this in the text.

158: At this point of the paper, it'd be nice to have a photo of the cable itself, as well as some photos showing it within the river. That could also go in the supplementary.

Figure 1b already includes a photograph of the cable along the bank; we have also added a larger version of this photograph to the supplement. We have added text to the Figure 1 caption to highlight this. Unfortunately the submerged portion of the cable is not visible in photographs.

174: I am not entirely sure why this was needed. Is it not possible to retrieve the cable positions with the orthophoto you are showing in Fig. 1b? assuming it was taken at the day of DAS deployment.

The orthophoto shown in Figure 1a is from Google Earth Pro collected in 2019, the most recent imagery available. Orthophoto collection during the deployment was not possible because drone flights are banned over Clear Creek. We have clarified this point by crediting Google Earth Pro in the Figure 1 caption.

182: how did you manually validate them?

We have reorganized this sentence to make clear that the methods described are those used for manual validation.

203: At this point the reader should be aware of the temporal deployment strategy. Since you mention 15 minutes deployment, which seems very short at first glance, why did you choose such time period? I think it would be good to state why such deployment may not be feasible for longer timescales.

We have added a note at the start of Section 3.1 mentioning the deployment length. However, we chose not to expand further on the monitoring duration, as it is not directly relevant to the study's

goals. Since this was a pilot study, the decision to monitor for 15 minutes was based on practical considerations and aligned with the study's objectives. This timeframe was adequate for capturing the stationary hydraulic behavior of the stream and does not reflect the feasibility of longer deployments, which was beyond the scope of this research.

207: what is relative depth?

Relative depth is the fractional depth at which velocity measurements are taken within the vertical profile of a stream. We have added a parenthetical aside to clarify this for readers unfamiliar with hydrology.

207: active channel width – do you mean the flow width?

Yes, active channel is the portion of channel that is wetted (the flow). We have also added a parenthetical note here.

222: why the choice to perform the analysis on 30 seconds? Why not also exploring a larger time window, of, e.g., 10 minutes (maybe it minimizes random noise)?

As noted in the text, we observed no differences when analyzing multiple 30-second segments throughout the acquisition period, which confirms that the selected segment is representative and unlikely to be affected by random noise. We did not use longer time windows due to computational limitations. Specifically, with a sampling rate of approximately 20 kHz and ~200 channels of data, the resulting datasets are extremely large. Processing continuous data over a durations of 10 minutes would far exceed our current computational capacity and was unnecessary for the current study.

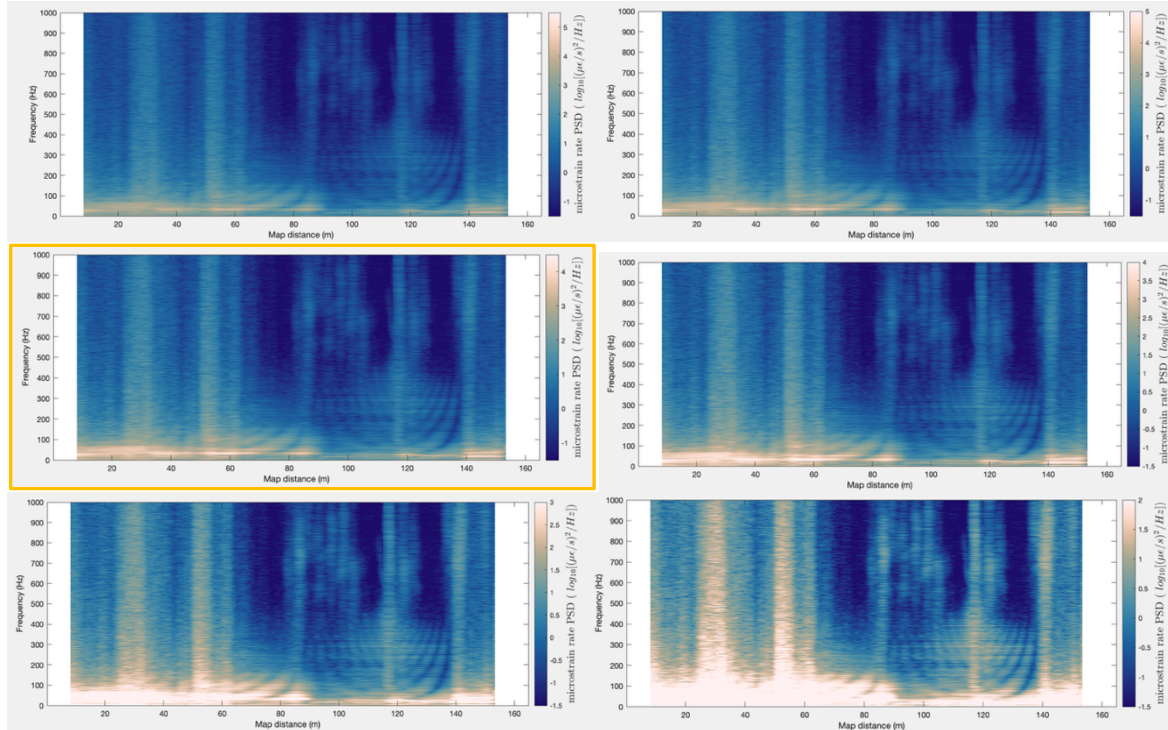
225: What do you mean by “amplitude spectra”? Are you referring to the Power Spectral Density (PSD)? The term “amplitude” typically implies taking the magnitude of the waveform, which I don’t believe is your intended meaning here, according to the next sentence. Please clarify and use these terms consistently, adhering to standard terminology.

We thank the reviewer for pointing out the need for clarity regarding terminology; however, when we refer to “amplitude spectrum” in this context, we do indeed mean the magnitude of the waveform. Specifically, we used standard methods to obtain the power spectral density (PSD) by calculating the amplitude spectrum using a Fourier transform, then squaring it and normalizing by the frequency bin width to manually obtain the Power Spectral Density (PSD) (e.g., via Parseval’s theorem; Stearns, 2002 - Digital Signal Processing with Examples in MATLAB.; etc). We have added language clarifying these details to the text.

229: This is a very nice and informative figure. However, I believe Fig. 1e could benefit significantly from a few adjustments: (i) Consider modifying the color scale or adjusting the saturation. At present, the PSD does not appear to vary much across different frequency bands, though this is likely an issue of symbology rather than actual signal content. (ii) Since elastic waves generated by fluctuating turbulent forces on the bed typically dominate the 1–100 Hz range (e.g., Gimbert et al., 2014), I strongly encourage you to include a similar spectrogram truncated at 100 Hz. My argument here is also reinforced by your line 248. The 1-100 Hz version could be included in the main text or, alternatively, in the supplementary materials.

We have adjusted the color saturation to a maximum of 4.5 rather than 5.5. We also note that we have previously conducted thorough experimentation with various color scale options,

including different color maps, limits, and normalizations, while considering only color maps that are perceptually uniform, ordered, and color-vision deficiency friendly to ensure reader accessibility. Among these, the current choice of color map was found to be the most suitable for minimizing saturation while effectively capturing all key features of the spectrogram (see screenshots below with alternate color bar upper limits leading to oversaturation of features below ~100 Hz; selected limit of 4.5 highlighted with yellow box). We also note that the supplement contains a version of the spectrogram on log axes, which we deemed to be the best alternative option for visualizing features challenging to discern in Figure 1e. We have also provided a zoomed in version of the spectrogram truncated at 100 Hz (on both log and linear y-axes) in the supplement (Fig. S2).



Lines 241-244: The description of your method as producing a "cross-correlation matrix representing along-stream wave coherence" is somewhat misleading. In signal processing, *coherence* typically refers to a frequency-domain measure that quantifies the consistency of phase and amplitude relationships between signals at each frequency (e.g., magnitude-squared coherence). In contrast, your method appears to involve computing time-domain cross-correlations between channel pairs, and extracting the maximum value across a range of lag times. If you did not explicitly compute spectral coherence (e.g., via cross-spectral density analysis), I recommend replacing the term "coherence" with "cross-correlation" or "signal similarity" to avoid confusion. If you did intend to quantify spectral coherence, please clarify the method used and how it differs from or complements the time-domain correlation matrix.

We have updated the text to reference signal correlation as a measure of self-similarity rather than coherence.

250: The phrase "regions of highly turbulent flows" is somewhat misleading. It appears that you are interpreting certain morphological features of the riverbed as indicative of high turbulence. While this may be a reasonable inference, it is not directly demonstrated in the data. As such, using morphological

characteristics as indirect evidence for turbulence without explicit validation could be misleading. I suggest rephrasing this statement to clarify the basis for your interpretation and to distinguish between observed morphology and inferred flow conditions. *And* 268-269: here is another instance where the manuscript refers to turbulence: "highly turbulent flow" (line 268) and "lower turbulence" (line 269). As mentioned in my earlier comment on line 250, since such terms are used repeatedly throughout the text, it would be helpful to briefly define what is meant by "turbulence" early in the manuscript. Additionally, please clarify how you qualitatively assess or distinguish between "low" and "high" turbulence in the context of your study.

We appreciate the reviewer's suggestion and agree that our original phrasing needed more clarity. We have revised the text in section 4.1 to clarify that our inference of turbulence is based on well-established hydrodynamic associations between morphology and turbulence and qualitative field observations, rather than direct measurements (which we lacked the instrumentation to collect at the time of this study). The revised wording clarifies that we are inferring turbulence from both prior literature and field observations of morphology and surface flow, while explicitly acknowledging that our dataset does not include direct turbulence measurements. We have also added more explicit discussion in terms of Froude numbers (which we are able to estimate quantitatively) to section 5.1 to supplement our discussion of turbulence.

256-257: Please clarify how you establish the association between coherence (or incoherence) and morphological features. While the general idea seems intuitive, the explanation is currently a bit unclear. For example, some boulder sections appear to correspond to spatial locations with relatively high coherence, which seems counter to the interpretation that turbulence leads to incoherent signals. Am I misunderstanding something, or is there additional context that could help clarify this relationship? A more explicit explanation of how morphological features map onto the coherence patterns would strengthen the argument.

As noted in the text, our data lack the spatial resolution and field measurements needed for detailed investigation at this scale, so we have focused our discussion on larger-scale features—namely, major correlation boundaries that align with previously documented drivers of wave incoherence and broadband power; we have also supplemented the discussion of these results in section 5.1 with the additional discussion about Froude numbers. With respect to the reviewer's suggestion that boulder sections correspond to high coherence, we do not observe this relationship. Although the cable passes within ~3–5 m of several large boulders in highly correlated regions, it does not approach them closely enough for their wakes to be expected to significantly decorrelate the signals under the observed shallow-flow conditions. It is possible that these boulders contribute to some of the variation in correlation within individual regions, but our data do not allow us to distinguish such effects from noise.

259: "upstream end" – please refer to the spatial (map) distance indicated in Figure 2.

We have updated the text to indicate this distance.

275: "upper pool and run". Where are these?

We have added text to remind readers that these regions correspond with the dotted blue and purple lines in Figure 1c-e.

276-281: These are valuable observations, but they remain largely qualitative in the current form. Why not make them more explicit? For example, consider adding a plot that summarizes spectral

statistics along the fiber cable, such as binned PSD values at selected frequencies plotted against distance. You could also annotate key features along the cable (e.g., rapids, boulder zones, runs) to highlight how the spectral properties vary spatially.

We appreciate the reviewer's suggestion and agree that visualizing spatial variation in spectral statistics is valuable. We believe this is effectively addressed in Figure 1c, which presents binned PSD values along the fiber cable and highlights spatial trends in spectral power. We have chosen not to further annotate Figures 1 and 2, as these figures are already densely annotated; additional annotations identifying rapids, boulders, and other channel features—which are already explicitly identified in the maps and legends (Fig. 1a, Fig. 2)—would obscure key patterns and introduce visual clutter.

275-282: The differences in turbulence-related spectral content you describe, such as the flattening at high frequencies, the broad peaks in the rapids (~15–60 Hz), and the sharper, narrower peaks in pools (~25–45 Hz and beyond), are particularly interesting. These patterns suggest meaningful variations in flow dynamics across channel features. Consider making a more explicit connection to recent work showing similar turbulence-induced spectral changes. For example, see <https://doi.org/10.1029/2024GL113784>, which documents comparable frequency-dependent spectral behavior linked to hydraulic complexity.

We have expanded the discussion in Section 5.1 to better relate our findings to recent work in fluvial seismology, including Nativ et al. (2025). We also reference established research on supercritical flow dynamics in rapids to contextualize the observed correlation differences among channel units. However, as noted in the text, our dataset lacks the fine-scale flow velocity measurements and precise cross-flow cable positioning needed to robustly interpret detailed spectral features, particularly given the along-cable signal propagation effects discussed extensively elsewhere in the manuscript. For these reasons, we have aimed to strengthen connections to prior observations without overextending interpretation. Finally, we emphasize comparison with hydrophone-based studies rather than seismic ones, since the DAS cable primarily records highly localized turbulence and is therefore more directly analogous to hydroacoustic rather than seismic sensors.

283-284: Could the high-frequency peaks be partially attributed to human activity during the experiment? For example, movements or equipment handling by personnel might generate high-frequency noise, especially if these actions occurred during data acquisition.

We observed no noticeable ongoing human activity in this region during the deployment, and the consistency of these observations across different time periods and average spectrograms throughout the deployment suggests that they cannot be attributed to random noise.

286: The x-axis in Video 1, labeled by map distance, is difficult to interpret, particularly since the range spans from approximately 440 m to 600 m. This makes it challenging to relate features in the video to those discussed elsewhere. Consider using the same x-axis configuration as in the main figures for consistency, and clarify the spatial reference to aid interpretation.

Thank you for pointing this out. We have redone the video with map distances matching the other figures throughout the manuscript, changed the marker line color to white on the spectrogram to increase visibility, and also added a marker to more clearly indicate the physical location of each spectrum on the Clear Creek map image.

291-292: I don't understand this sentence. Rephrase.

We have clarified the text and referenced the dashed blue line at map distance 117 m on Figures 1c and 1e to better orient the reader here.

294: out of context. Do you mean regions apparent in Figure 2?

We have updated the text to clarify that this references the previously discussed coherent regions in Figure 2.

296-297: The phrase “three visible bands increase in frequency with distance upstream” is unclear. Do you mean that the power within those frequency bands increases, or that the central frequency of each band shifts? As written, it's ambiguous, please clarify. Also, when trying to interpret this in the spectrogram (1e), the figure would benefit significantly from adjusting the color saturation. The current color scale makes it difficult to distinguish finer spectral features.

We have updated the text to clarify that we are referring to shifts in the central frequency of each band. As previously noted, we have adjusted the color saturation in Figure 1e and also followed the reviewer's suggestion to provide the spectrogram (again, on log and linear y-axes) up to only 100 Hz for a more detailed view in the Supplement (Fig. S1).

324-325: The term “maximum frequency” is ambiguous here. Do you mean the upper frequency limit of the inertial subrange, or the frequency at which the PSD reaches its maximum? Please clarify what this refers to in the context of your analysis.

We have updated the text to clarify that this refers to the maximum frequency of the inertial subrange.

343: my above comment also refers to this line (“peak frequency”).

We have updated the text to clarify that this refers to the peak frequencies discussed in section 4.1.

393: Since there is no Figure 2b, I assume you're referring to a different figure, perhaps Figure 3? Please clarify and correct the reference.

Reference has been corrected to Figure 3b, thank you.

398-399: The paragraph would benefit from a clearer statement of the authors' objective at the outset; what is the goal of identifying "knocking" events and modeling their arrival times? Additionally, the nature of these events is not well defined: what exactly constitutes a "knocking" event, and how were they identified in the DAS data? A clearer explanation, perhaps in the supplementary materials, would be helpful. The description of the grid search used to optimize wave propagation velocity and source-to-cable distance also lacks important details. Please clarify which parameters were varied, what objective function was used, and how observed and modeled arrival times were compared.

Thank you for the suggestions; we have made these points more explicit in revision. The purpose of identifying “knocking” events and modeling their arrival times is to locate the impulsive sources and to infer the wave propagation velocity. These constraints allow us to interpret the physical origin of the signals and relate them to the fluvial processes discussed in the paper—namely, to test whether the impulses might in fact be generated by otherwise unobserved sediment motion. We have more clearly defined our criteria for “knocking” events and the optimization processes in the text.

Figure 4a: The panel lacks a clear explanation of what is being shown. Please clarify what the individual traces represent and explicitly describe the meaning of the black and white colors, do they correspond to signal polarity, amplitude, or something else? A clear legend or explanation in the caption or main text would make this panel easier to interpret.

Figure caption has been updated to note that the traces represent strain rate waveforms from each DAS channel and black fill indicates positive strain amplitude.

Panel c: Does the distance shown correspond to the z term in the equation on line 404? If so, please make this explicit. While lines 406 - 407 seem to make that connection, it should already be clear at this point in the text, ideally through the caption for Figure 4.

Clarification has been added to the caption for Figure 4.

414: please add the actual propagation of sound in water, for reference.

We have moved this reference to this location.

417-420: Please clarify how the results from the three grid search scenarios described in the text map onto the labels used in Figure 4's legend. It's currently difficult to determine which curve or bar corresponds to each modeling approach. For example, in line 420 you state that the velocity was fixed at 1450 m/s, which led to substantially higher misfits. However, in Figure 4d, the "velocity optimized" case appears to have the lowest RMS error, suggesting a mismatch between the figure and the textual description. This discrepancy should be resolved to avoid confusion.

"Optimization" refers to the variable being varied to obtain its optimal (i.e., best-fit model) value. We have added references to specific bar colors in Figure 4 to clarify that the "velocity optimized" case refers to the red bars, whereas the scenario the reviewer mentions here is the "distance optimized" case shown with yellow bars.

425: Please briefly explain why you are attempting this, even though no grain motion was observed.

As noted in the revised text, this calculation was included as a precautionary check in case the signal reflected sediment motion that was not visible in the field. To maintain focus in the main text, we have moved the detailed description of this calculation to the Supplementary Material (S1).

452-458: I'm having trouble following your observations in this section. Could you clarify where exactly you're seeing this? I'm not able to track it clearly with Figure 1. Please elaborate.

We have added a new supplemental figure (S7) to more clearly demonstrate these observations, and have expanded the text to be more explicit.

460: The rationale for this modeling approach isn't clear to me. Could you explain the aim and purpose of modeling the signals in this way?

We have added text to clarify that this modeling approach was intended to test the hypothesis that the pulse train is responsible for the spatio-spectral gliding.

464 - 471 / Figure 5b: The modeling uses time-domain wavelets with amplitude $A(t)$, but the output shown in Figure 5b is PSD. Please clarify the processing steps used to derive the PSD from the

modeled waveforms. Was a spectrogram computed? This link between time-domain modeling and frequency-domain results should be made more explicit.

We have updated the text to clarify the steps used to go from the time domain microstrain rate model data to the model PSD, which were identical to those used on the original DAS data to produce figure 1e.

Figure 5b: The caption mentions an adjusted color scale, but it remains unclear what the colors specifically represent. Please include a legend or annotation indicating the units or range of the power spectral density values.

We have added a colorbar to Figure 5. We now also show both the spectrograms composed of detrended spectra (Fig. 5b) and the raw spectrograms prior to detrending (Supplemental Figure S7).

472: What is a pulse train? What do you mean by pulse shape?

“Pulse train” is previously defined in the first sentence of this section as “a series of lagged pulses, also known as a pulse train or comb,” which we have updated to include “(discrete, transient signal)” for clarity. We have also clarified the meaning of “pulse shape” by noting that this means the shape of the pulse waveform.

Section 5.4.2: Given the manuscript already presents a substantial number of complex observations and interpretations, I am not sure this paragraph meaningfully contributes to the overall narrative, in fact, it risks making the section heavier for the reader. Moreover, since the authors validate through both visual observations and independent analysis that no sediment transport occurred, the speculative discussion of bedload-generated signals may be redundant in this context. I suggest considering its removal or significantly condensing it.

We have significantly condensed and shortened this section.

573-574: I don't follow this argument . Please explain.

We have rewritten this sentence more clearly.

615-616: It is unclear how a resonance mechanism would be consistent in this context. The argument does not clearly explain this point. Additionally, the rationale for referencing compound 3b is not evident.

We have clarified the text on this point and removed the out of date reference to Figure 3b.

Section 5.5.2 (Lines 677–698): This section offers a helpful overview of potential strategies for bed-surface DAS deployments, addressing both challenges and practical solutions. However, it reads more like a list of ideas than a focused discussion. Consider streamlining the section by emphasizing the most promising approaches and more clearly separating well-supported methods from speculative ones.

Thank you for this suggestion. We have revised the section to emphasize the most promising, well-supported strategies (e.g., rigid pipe/conduit systems and heavy cables) while clearly distinguishing them from more speculative considerations, thereby improving focus and clarity.

Reviewer #1

This manuscript presents a novel and well-executed in-stream deployment of distributed acoustic sensing (DAS) to monitor high-resolution seismo-acoustic signals in a natural river. The authors deploy a fiber-optic cable across Clear Creek, CO, and use DAS to identify coherent frequency bands, turbulence-related broadband noise, and a compelling spatial-spectral gliding phenomenon. The experimental setup, methods, and analysis are detailed and convincing. The interpretation of the “knocking” signal is particularly strong, and the discussion of alternative mechanisms is thoughtful. The work offers a valuable reference for future DAS deployments in fluvial environments. While the focus is primarily hydraulic, the application of DAS is highly relevant to seismology. This work may also appeal to readers in hydrology or geomorphology. I leave the question of journal fit to the editor’s discretion.

Minor comments:

The caption for Figure 1 appears truncated.

This has been corrected, thank you!

When using travel times to grid search for velocity and distance, consider plotting a 2D grid with velocity and distance as axes and misfit as color or amplitude. This would make the trade-off between parameters more visually intuitive. As it stands, Figure 4b–d is somewhat difficult to interpret.

We thank the reviewer for this helpful suggestion. We have added the recommended figure to Figure 4 (part b), and agree that it makes the parameter trade-off significantly more visually intuitive.

The spatio-spectral gliding signals in Section III are attributed to knocking and reflection. What about the gliding features observed in Section I (upstream rapid)? Are they explained by the same mechanism, or do they require a different interpretation?

As briefly discussed at the very end of section 5.3, these bands are too indistinct to attempt to model. However, we do observe partial reflections of knocking signals here, suggesting that some knocking signals transmit downstream beyond the point of partial reflection and produce the same phenomenon. We have updated the wording in this section to make it clear that we also attribute the gliding downstream of the reflection point to the same mechanism.