Review for egusphere-2024-4111, title: A physical model for mean river discharge calculation: from riverside seismic monitoring experiments in a low flow river, China

Manuscript egusphere-2024-4111 entitled “A physical model mean river discharge calculation: from riverside seismic monitoring experiments in a low flow river, China” by Xiaoyue Zhou and co-authors investigates the relationship between seismic signal, streamflow properties and human-generated seismic noise along a reach of the Jiuqu river, China. The manuscript notably proposes a model to predict discharge based on the seismic energy recorded in the lower frequency domain (e.g. < 10 Hz).

I am not recommending to pursue the revision process of this manuscript in its current form, because I think it is too far from the standards of the journal Earth Surface Dynamics. A publication of the manuscript would require in my view a clearer definition of the study aim, a clearer identification of the knowledge gap to fill with regards to the literature, and clearer description of the methodology. In addition, the presented results are either not new (e.g. seismic energy related to turbulence in the lower frequency domain) or not convincing enough (e.g. the performance and necessity of the proposed empirical model). I am reporting general comments, specific comments and technical corrections in the section below to help the authors to follow my review and revise the manuscript.

General comments

The general structure of the manuscript is not satisfying. There is no clear cut between methodological considerations, manuscript results and result discussion, which are mixed up through sections 3 to 5.

The introduction does not justify well enough why the microseismic sensing of the streamflow is valuable as compared to more traditional hydrological stations or remote sensing methods. Those different approaches and their respective strengths and weaknesses should be more clearly identified in the introduction, with appropriate referencing, to better capture the rationale of the manuscript, which remains generally unclear. In this context, I would focus on streamflow seismic sensing – the main scope of the manuscript – and leave apart considerations about bedload transport seismic sensing.

The description of the experiments in the second section does not provide with enough details, and some more information about the set-up of the different experiments would be valuable to better follow the procedure, and for a matter of reproducibility. In addition, the distinction between different cross-section, experiments and tests is not always clear to me and makes the manuscript difficult to read. More importantly, the purpose of the different experiments should be made much clearer at an earlier stage, already at the end of the introduction, so that the reader exactly knows what will be done and why to tackle the research question.

Some of the presented results – e.g. seismic frequency domain of the water turbulence – are not new (e.g. Schmandt et al., 2013). In addition, I am not convinced that the precision of the flow velocity measurements really allow to use this dataset to test the performance of the seismic discharge model. In particular because there are so little variations in discharge during the experiment, as well as possible contamination of the seismic signal by people walking in the river to perform the flow velocity measurements (unless I miss something in the Method section). The proposed model to predict discharge based on seismic sensing is empirical in my view, since it requires a calibration based on local measurements, and is therefore not physical as stated in the manuscript title. Instead, there is already a seismic physical model of turbulence (Gimbert et al., 2014) that has been published and tested (e.g. Dietze et al., 2019), and I am therefore wondering what the proposed model brings in comparison.

I find the bimodal distribution of seismic power (Figure 10a) in the frequency bands 2-15 Hz and 35-50 Hz for the steeper and potentially more turbulent section to be an interesting result. Effectively, this shows that there may be more overlap between turbulence and bedload transport frequency bands for more turbulent river, which may be an issue to invert bedload transport rates in this type of settings. I would suggest the authors to reorient the scope of their manuscript in this direction.

In general, the grounding of the manuscript into an appropriate literature context is insufficient, so is the mobilization of the literature to illustrate and support the argument, and to put the results into perspective in the Discussion. In many instances, the used references are irrelevant and/or erroneous with regards to the treated topic (e.g. reference to Aderhold et al., 2015 on l. 101-104, or to Bagnold et al., 1966 on l. 325). There are some articles in the bibliography that are not cited in the manuscript.

Specific comments

1. Introduction

l. 33-40: I do not find the first paragraph to be very efficient to justify the need for microseismic studies of water discharge.

l.36: Please add a few references to illustrate your argument about remote sensing technology.

l.38-39: Cook and Dietze (2022) and Larose et al. (2015) have reviewed environmental seismology. They may not be the best references to illustrate your point about time-consuming and resource-intensive approach of hydrological stations.

l. 41: Please define what you mean by “microseismic”.

l.42-43: Turowski et al. (2011) have not used environmental seismology, but acoustic sensors.

l. 46: Rickenmann et al. (2012) have used acoustic monitoring of bedload transport, not microseismic.

l. 57-69: The focus should remain on water depth/discharge seismic sensing, and not much on bedload transport sensing, since it is not something done in the frame of this manuscript.

l. 69: Gaeuman (2014) is an off-topic reference.

l. 71-78: The paragraph goes in too many directions, without a clear structure, and lacks of appropriate scientific references.

l.76-77: I am surprised that the turbulence model of Gimbert et al. (2014) to invert streamflow information from low frequency band seismic energy is not mentioned, described and further mobilized here, and throughout the manuscript.

l. 87: Not convinced Viparelli et al. (2011) is an appropriate reference in this context.

2. Experiments

l. 91-97: Characteristics and statistics that are provided to describe the field site are not backed up with references.

l. 97-98: Can you provide some statistics on the discharge of the Jiuqu river (annual mean, variability, discharge during the experiments, etc.). Also, why is the discharge different between the different sections (e.g. tributary input?). If assuming mass conservation, the discharge is constant between the different sections, and it is its partitioning between width, depth and flow velocity that varies.

l. 97-101: At line 97, it is mentioned that "four monitoring experiments were conducted at four sections (…)". At lines 99-100, "we selected a curved section of the Jiuqu". I do not understand the link between the initial four sections, and the following one section.

l. 101-102: the description of the channel morphology could be clearer. What is meant by "drainage canal"?

l. 101-104: the river studied in Aderhold et al. (2015) is in New Mexico! How do you transfer their local grain-size distribution measurements to the Jiuqu River, China?

l. 105-107: Is this information about silt and sediment concentration needed with respect to streamflow seismic sensing? If it is, please justify its purpose in the text. If not, I would remove it.

l. 107-108: Would be good to have some information about mean annual streamflow (cf. comment l.97-98), to be able to scale it with respect to the streamflow during the experiment. Please add a reference to the concept of "low-flow river".

l. 108: The morphology of the different sections should be described more extensively than what is provided in Figure 2 or along l. 101-102. Why are those different channel morphologies selected with respect to the research question?

l. 119: Location of the seimic sensor is not clear.

l.119-122: I would say this information is not needed.

l. 123-124: Phrasing not clear enough with respect to which of the two types is sensitive to high-frequency (…). Please rephrase.

l.125-132: I am not convinced the content of this paragraph is of first relevance with regards to the focus of this manuscript.

l. 142-143: why isolating the sensors from the ground if seismic waves travelling in the ground are aimed to be measured?

Figure 3: I am struggling to follow between the four test sites in Figure 1, and the four seismic stations in Figure 3. Please clarify how many seismometers were used in each site, where there were located, what distance to the stream, what were the potential sources of external noise, etc. Perhaps a summary table would be helpful for this purpose.

l. 154-155: According to Figure 3, S3 is located about ~20 m from the riverbed, not 1 m.

l. 158: Is an 'experiment' the same thing than a 'test'. Please use the same word to refer to the same step, to easen the reading.

l. 163-169: This paragraph may belong to the 'Result' section.

Figure 4: I am surprised you measured no seismic signal related to turbulence at S4 and S5, while you remain very close to the river. It would be good to have the same y-scaling on the all four velocity plots. Isn't it also a problem of scaling that the seismic noise associated to turbulence does not appear on spectrograms where external noises are also present?

l. 182: Unclear what is meant by 'appropriate devices'. Please be more specific, with argument illustrated by references.

Figure 5: I am not sure the diagram is of first relevance with respect to the scope of the manuscript. It could go in a Supporting Information.

Equations (1) to (7): Please specify the units of every used variables.

3. Seismic ambient noise

l. 220: It is not clear if there should be the 'Results' section starting here, or whether we are still in the methodology description.

l. 223: Reference to back up your statement.

l.224-225: Not clear what "flow configuration of the river" means in this context. Please be more specific.

l. 227: Reference to back up your statement.

l. 228-231: In this paragraph, I would really specify what process can be distinguished (again, focusing on streamflow) in which frequency band, with referencing to the appropriate literature.

l.241-246: This paragraph may belong to the 'Results' section.

l.246-248: More justification about the signal filtration would be valuable (e.g. further arguments, a figure, some references).

Figure 6: as for Figure 4, it would be good to have the same y-scale on both ground velocity plots to ease the reading.

l.262: It is still not clear to me whether the different Tests are used to investigate the same thing at different sites, or whether different experiments are conducted at each site.

l. 272-276: Comparison of the 'Results' with the literature belongs to the Discussion section I think.

l.278-280: This is effectively no new result (e.g. Gimbert et al., 2014).

l. 283: Please be consistent and specific with the naming. What are locations 1, 2 and 3? According to l. 256-270, location 1 should instead be Test 1, S4; location 2 should be Test 2, S2, etc. I agree this does not read well, so I think there is a general rework to do to rename all the experiments and sensor location in a clearer way.

l. 293-297: Mainly repetitions from l. 221-227.

l.298-299: Those considerations should come earlier in the manuscript. The reference to Boano et al. (2011) is irrelevant (i.e. the paper is about hyporheic exchange, not sediment transport).

Figure 9: This is all known, not needed.

l. 312-315: This is an interesting result, that you get a bi-modal distribution of seismic energy for the river section that is more turbulent.

l.317-328: Again, I would say those considerations belong to the Discussion.

l.320-322: But you were in the field during the experiment. Was there any bedload transport going on? Perhaps your result shows that more turbulent section actually produce seismic energy in a higher frequency band, which partly overlaps with the frequency band we typically attribute to bedload transport.

l. 325: Why do you mention human activities here? Reference to Bagnold (1966) is inappropriate in this context. Not convinced the reference to Turowski and Bloem (2016) is neither relevant in this context.

l.331-339: Please specify in the Figure 10 caption for which Test and sensor location the different diagrams relate to.

4. Seismic interpretation and river discharge calculation

l.342-364: This is pure methodological description, and it should appear much earlier in the manuscript. You have already presented many result figures containing PSDs.

l.370-374: Assuming that the width and depth are constant then? But if there are changes in flow velocity, there may be changes in width and depth too, right?

l.378-381: I do not find the matching that clear. You may want to use a metric like a correlation to support your observation more quantitatively. The variations in discharge are very small, probably much smaller than the precision you get with the hydraulic estimate of discharge, so I am not convinced those results are robust enough. In addition, I guess your were walking into the river to do the velocity measurements, and this may have also been recorded in the seismic signal.

Figure 11: Why is it needed to present the results for every 3 components of the seismic sensor?

5. Results and Discussion

l.396-397: Please specify what you mean by “without considering the mechanical effects generated by the river process”.

l. 397-403: Again, not clear from which experiment, test, location, section, etc. we are talking about.

l. 403-404: Not clear to me.

l.420-421: How were the parameters of the Green's function estimated?

Figure 12: Same question than for Figure 11: why is it needed to present the results for all 3 component of the seismic sensor? It is not clear to me what is presented in this Figure.

l.434: Why those frequencies in particular?

l.437-439: I do not see any comparison between the seismic prediction of discharge and the flow calculation using the flow velocity measurement in Figure 12. Perhaps an issue related to the labelling of the Figure? A plot showing measurements vs predictions would be a more efficient way to assess the performance of the model (as effectively done in Figure 13).

l.442-443: How the reported error magnitude compares to the variability in discharge during the experiments? I think the precision in the measurements using flow velocity does not really allow to test robustly the seismic model.

l. 466-472: In general, it looks like you are proposing an empirical model (and not a physical one as argued in the title) that requires calibration to predict discharge, while a physical model of turbulence has already been developed (Gimbert et al., 2014) and tested in multiple instances (e.g. Dietze et al., 2019), so I am not convinced by the usefulness of the proposed model.

6. Conclusion

l. 474-476: Again, not clear terminology to describe how the seismometers (number and location) are deployed.

l.477-479: It is expected that there are no correlation between the flow and seismic power at higher amplitudes.

l. 481: In what Result Figure do you observe a linear relationship?

l.481-483: This is not really a result from this study.

l. 487-503: The organization of the argument in this paragraph is very fuzzy.

7. References

l. 516: Burtin et al. (2008) not cited in the manuscript, while it definitely should.

l. 536: Foulds et al. (2014) not cited in the manuscript.

Technical corrections

l. 33: "usually include encompass (…)". Two different verbs saying the same, no?

l.57-58: "the vibrations of river sediment". Wording not ideal.

l. 91: "the river studied in this study". Poor wording.

l. 176: "precision" instead of 'accuracy' I think.

l. 510-586: in multiple instances (e.g. Schmandt et al., 2013), I saw that there are missing spaces between words in the references. Please check and correct throughout.

l. 563: Reference not sorted in alphabetic order.

l.275: Tsai et al. (2012), and not Tasi.

l. 568: Tsai et al. (2012) cited twice in the bibliography.

References

Aderhold, K., Anderson, K.E., Reusch, A.M., Pfeifer, M.C., Aster, R.C., Parker, T., 2015. Data Quality of Collocated Portable Broadband Seismometers Using Direct Burial and Vault Emplacement. Bulletin of the Seismological Society of America 105, 2420–2432. https://doi.org/10.1785/0120140352

Bagnold, R.A., 1966. An approach to the sediment transport problem from general physics. US government printing office.

Burtin, A., Bollinger, L., Vergne, J., Cattin, R., Nábělek, J.L., 2008. Spectral analysis of seismic noise induced by rivers: A new tool to monitor spatiotemporal changes in stream hydrodynamics. Journal of Geophysical Research: Solid Earth 113. https://doi.org/10.1029/2007JB005034

Cook, K.L., Dietze, M., 2022. Seismic Advances in Process Geomorphology. Annual Review of Earth and Planetary Sciences 50, 183–204. https://doi.org/10.1146/annurev-earth-032320-085133

Dietze, M., Lagarde, S., Halfi, E., Laronne, J.B., Turowski, J.M., 2019. Joint Sensing of Bedload Flux and Water Depth by Seismic Data Inversion. Water Resources Research 55, 9892–9904. https://doi.org/10.1029/2019WR026072

Foulds, S.A., Griffiths, H.M., Macklin, M.G., Brewer, P.A., 2014. Geomorphological records of extreme floods and their relationship to decadal-scale climate change. Geomorphology 216, 193–207. https://doi.org/10.1016/j.geomorph.2014.04.003

Gaeuman, D., 2014. High-Flow Gravel Injection for Constructing Designed in-Channel Features. River Research and Applications 30, 685–706. https://doi.org/10.1002/rra.2662

Gimbert, F., Tsai, V.C., Lamb, M.P., 2014. A physical model for seismic noise generation by turbulent flow in rivers. Journal of Geophysical Research: Earth Surface 119, 2209–2238. https://doi.org/10.1002/2014JF003201

Larose, E., Carrière, S., Voisin, C., Bottelin, P., Baillet, L., Guéguen, P., Walter, F., Jongmans, D., Guillier, B., Garambois, S., Gimbert, F., Massey, C., 2015. Environmental seismology: What can we learn on earth surface processes with ambient noise? Journal of Applied Geophysics 116, 62–74. https://doi.org/10.1016/j.jappgeo.2015.02.001

Rickenmann, D., Turowski, J.M., Fritschi, B., Klaiber, A., Ludwig, A., 2012. Bedload transport measurements at the Erlenbach stream with geophones and automated basket samplers. Earth Surface Processes and Landforms 37, 1000–1011.

Schmandt, B., Aster, R.C., Scherler, D., Tsai, V.C., Karlstrom, K., 2013. Multiple fluvial processes detected by riverside seismic and infrasound monitoring of a controlled flood in the Grand Canyon. Geophysical Research Letters 40, 4858–4863. https://doi.org/10.1002/grl.50953

Tsai, V.C., Minchew, B., Lamb, M.P., Ampuero, J.-P., 2012. A physical model for seismic noise generation from sediment transport in rivers. Geophysical Research Letters 39. https://doi.org/10.1029/2011GL050255

Turowski, J.M., Badoux, A., Rickenmann, D., 2011. Start and end of bedload transport in gravel-bed streams. Geophysical Research Letters 38. https://doi.org/10.1029/2010GL046558

Turowski, J.M., Bloem, J.-P., 2016. The influence of sediment thickness on energy delivery to the bed by bedload impacts. Geodinamica Acta 28, 199–208. https://doi.org/10.1080/09853111.2015.1047195

Viparelli, E., Gaeuman, D., Wilcock, P., Parker, G., 2011. A model to predict the evolution of a gravel bed river under an imposed cyclic hydrograph and its application to the Trinity River. Water Resources Research 47. https://doi.org/10.1029/2010WR009164