Manuscript ID: egusphere-2024-2525
Original Title: *Improving prediction of particle size with a novel acoustic bedload monitoring system consisting of phased microphone arrays and accelerometer*
Authors: Zheng Chen et al.

Dear Reviewer #1,

We sincerely thank you for your positive and constructive feedback. We are grateful for your recognition of the novelty and potential of our acoustic bedload monitoring approach using phased microphone arrays. Following your suggestions, we have made moderate revisions to improve the clarity and completeness of the manuscript. All changes have been highlighted in the revised version, and detailed point-by-point responses are provided below.

Responses to the Comments from Reviewer #1

- **General comment 1:** I want to congratulate the authors on a novel experiment using a microphone array to improve impact-based bedload surrogate systems. Their work represents an incremental, but crucial, step towards an effective instream surrogate system. However, I believe that the manuscript, as written, requires some moderate changes before it is ready for publication. I have detailed those changes below with relevant line numbers.
  The paper has a number of grammatical errors that should be corrected. I note some of them below in my line-specific comments but also wanted to add a general comment here. This is not at all a criticism of the lead author. Rather, I believe that the coauthors that are responsible for review and supervision should take a more careful review before resubmission of the manuscript. This is particularly noticeable in the introduction and methods sections of the manuscript.

  Response: We fully agree with the reviewer's comments which are important and essential for improving this manuscript. After restructuring and revising the content, we will proofread the entire text carefully and perform a thorough grammar and language check before resubmitting it. We will pay particular attention to the Introduction and Methods sections.

- **General comment 2:** Many of the figures in the results section appear before they are referenced in the manuscript. It appears intentional, but it's not typically how papers are structured in my experience. I defer to the preference of the editorial staff, but would like to see the figures referenced in-text before the associated figure is shown.

  Response: We thank the reviewer for this observation. In the original manuscript, some figures were placed before they were referenced to minimize page breaks between the text and the corresponding figures and thus improve readability during the review process. In the revised manuscript, we have adjusted the figure placements to ensure that all figures are referenced in the text before they appear.

- **General comment 3:** I think the discussion may benefit from some reorganization. My preference would be sections reorganized as follows: 4.4, 4.1, 4.3, 4.2. This would approximately match how the results are organized (number of microphones, microphone location, centroid frequency, grain size).

  Response: We appreciate the reviewer's suggestion regarding the organization of the Discussion section. We agree that rearranging the subsections in the order proposed (4.4, 4.1, 4.3, 4.2) improves alignment with the sequence of the Results section thereby enhancing the manuscript's logical flow. Accordingly, we have reorganized the Discussion section to follow this suggested structure in the revised manuscript (lines 498, 511, 595, and 637).

- **General comment 4:** In additional to general comment #3, I would appreciate some discussion of how this new system may perform under field conditions. How would the PMA system work with more than two impacts at once? In principal, the system should be able to better resolve these impacts based on your laboratory results, but I think a discussion of this is warranted. This technology is intended for deployment in the field to improve our understanding of actual bedload flux. A new subsection should recognize this, discuss the similarities and differences between systems already in place, and how this system may fair.

Response: We thank the reviewer for this important and forward-looking suggestion. We fully agree that natural bedload transport often involves multiple particle impacts that occur simultaneously or nearly simultaneously, and that the PMA system's ability to resolve such conditions is critical for its future deployment in the field.

In response, we have significantly expanded the discussion in Sect. 4.3 ("Spatial resolution of the PMA system and multi-source detection") to address the system's ability to detect multiple, simultaneous impacts. In addition to the two-source simulations, we have now conducted numerical tests involving three and four concurrent impact sources under symmetric and asymmetric conditions, and will include the results in the text (see Appendix B). These results provide a more realistic assessment of the system's performance under multi-source interference.

Regarding the question to compare the PMA system with existing technologies, the signal response characteristics of the PMA and the Swiss plate geophone (SPG) systems are compared in Sect. 4.2, highlighting differences in sensor placement, signal type, and sensitivity to impact location.

We acknowledge that field deployment is the ultimate goal of the PMA-technology. To this end, we are designing systematic flume experiments that will include multi-particle impacts under active flow conditions. These tests will serve as an important intermediate step between idealized laboratory conditions and field deployment, allowing us to refine and validate the system in more complex and realistic environments.

We also add a forward-looking paragraph to the end of Sect. 4.4 ("Improved estimation of particle size and outlook to field application"), discussing the outlook for field deployment.

While the reviewer suggested adding a dedicated new subsection, we believe this topic is best integrated into the relevant existing sections for clarity and to avoid fragmentation. Therefore, the discussion of multi-source detection has been incorporated into Sect. 4.3, and a forward-looking paragraph on potential field implementation has been added into Sect. 4.4.

- **Specific comment 1:** Lines 1-3: I think the title is a bit misleading. By my understanding, the improved particle size is just one of the improvements described in the manuscript. I would highlight the novelty of PMA system as a whole in the title.

Response: We thank the reviewer for this helpful comment. We agree that the current title may place too much emphasis on the improvement in particle size prediction, whereas the manuscript introduces a more comprehensive innovation—the development and testing of a new PMA-based acoustic monitoring system. Accordingly, we have revised the title as follows:

"A novel acoustic bedload monitoring system with phased microphone arrays and accelerometer for particle localization and size estimation"

- **Specific comment 2:** Lines 18-19: This sentence structure is a little confusing - is it a single regression or different regressions between impulses and the rest of the variables?

Response: Thank you for pointing out this ambiguity. To clarify, we performed separate regression analyses examining the relationship between particle size and each of the following signal characteristics: the number of impulses per particle mass, the signal amplitude, and the centroid frequency. In the revised manuscript, we have rephrased the relevant sentence to make this distinction clear (lines 18-19).

- **Specific comment 3:** Lines 15-16: What is the 'conventional beamforming method'? - you describe it below so I think just referencing the section below is sufficient here.

Response: Thank you for raising this point. We agree that the term should be clear to the reader. Upon consideration, we have opted to keep the abstract stand-alone without internal cross-references, as is usual practice. To address your concern, we have ensured that the term 'conventional beamforming' is explicitly explained and detailed in a dedicated subsection (Sect. 2.3) within the methods section. We believe this provides a clear pathway for interested readers to easily locate the full technical description.

- **Specific comment 4:** Line 50: other studies have investigated the effect of grain size when using impact sensors, such as Halfi et al., 2021 or Stark et al., 2024

Response: Thank you for the suggestion. We agree that these studies are relevant to investigating bedload grain-size effects in impact sensor systems. We have added these references and cited them appropriately in the revised manuscript, where we discuss previous work on sensor type, particle size and signal characteristics (lines 35 and 50).

- **Specific comment 5:** Lines 89-91: I don't think that specifying the manufacturer is necessary. Keeping it in would be fine though.

Response: Thank you for pointing this out. We have decided to keep the manufacturer information in this section, as this may help interested readers to identify and compare the specific equipment used in the experiments. This could make it easier to reproduce the experiments and explore similar instrumentation further.

- **Specific comment 6:** Lines 100-102: are these calibration coefficients universal across all systems, or is this established for every system prior to use?

Response: Thank you for raising this important point. The calibration coefficients provided in lines 100–102 of the original manuscript are manufacturer-specified constants for the specific sensor models used in our study. For sensors of the same model, these coefficients are generally consistent. However, small variations may occur due to manufacturing tolerances.

It is also important to note that these calibration coefficients are only valid within a specified frequency range. For example, the microphone sensor has a typical frequency response from 10 Hz to 20 kHz, whereas the accelerometer operates effectively within the range of approximately

0.5 Hz to 10 kHz. Outside of these ranges, frequency-dependent calibration curves may be required for accurate signal conversion.

We have clarified this point in the revised manuscript (lines 101-103).

- **Specific comment 7:** Line 113: The use of eccentric seems unusual here, but is technically correct. I think something like center and off-center may be more intuitive to the reader, rather than centric and eccentric, but accept that there is nothing wrong with the word choice. Still, I would appreciate you explicitly state what a centric and eccentric impact entails here, in the methods, rather than waiting to the results to define it.

  Response: We appreciate this helpful suggestion. While the terms "centric" and "eccentric" are technically correct, we agree that they may not be immediately intuitive to all readers. To improve clarity, we have added a brief explanation to the Methods section. This specifies that a centric impact refers to a particle colliding at the center of the PMA plate and that an eccentric impact refers to an impact at a more off-center location. We have kept the original terminology throughout the manuscript, but have clarified its meaning when it is first used (lines 120-121).

- **Specific comment 8:** Lines 113-114: You note that you conducted the experiments in both air and water, but it isn't clear in my reading of the results which you report. Is it both?

  Response: Thank you for pointing this out. As mentioned in the Methods section, the experiments were conducted in both air and water. The water-based tests were intended as supplementary experiments. As the impact velocities in air were generally higher and produced stronger signal responses, we have chosen to present and analyze the results from the air experiments in the manuscript. This point has been clarified in the revised text (lines 126-128).

- **Specific comment 9:** Lines 125-126: Each section (such as this one) often begins with a statement of what the section is intended to accomplish. I don't think this is strictly necessary, and could be removed for brevity but if the authors feel strongly or prefer this, then I think it is fine to keep them!

  Response: Thank you for this suggestion. As we looked into this, we realized that some of the introductory sentences could be more concise. While we have kept them for clarity, we have revised and shortened them to improve readability and flow.

- **Specific comment 10:** Line 216: Are these all the relevant parameters or just some of them? It appears to be all, and if it is not, then the table should be expanded to include the parameter set used in the numerical simulations.

  Response: We thank the reviewer for this careful observation. The parameters listed in Table 2 are the key parameters used in the numerical simulations. Other parameters—such as microphone element spacing and array arrangement—vary across different simulation scenarios and are therefore not included in the table.

  To avoid confusion, we have revised the corresponding sentence in the manuscript to clarify that Table 2 contains only the key or representative parameters (line 242).

- **Specific comment 11:** Line 257: Are centric and centroid frequencies the same thing? I don't think so, based on my understanding of the paper. Please ensure it is centroid frequency and centric impacts

  Response: Thank you for pointing out this distinction. We have carefully reviewed the manuscript and revised the text to ensure consistent and correct terminology is used: *centroid frequency* refers to the frequency metric calculated from the signal spectrum, while *centric impact* refers to particle impacts at the center of the PMA plate. Please see lines 284, 286-288, and 726 of the revised manuscript for details.

- **Specific comment 12:** Lines 290-292: I think it would help the reader to include the microphone locations in Figure 6. Perhaps an inset or an overlay somehow? If not, you should reference the Appendix Figure showing the different arrangements here.

Response: We appreciate the reviewer's suggestion. To improve clarity, we have now added the locations of the microphone elements directly to Figure 6a–d as overlays. This allows readers to better understand how the reconstructed acoustic images s correspond to different array configurations. We believe this change makes the figure easier to interpret.

- **Specific comment 13:** Line 296: Section 3.1.3 -- You only reference the multipole results a single time in your discussion. I do not think this is necessary to be included in this manuscript. To be clear - I think that multiple sources is critical for using this surrogate technology (detailed in another comment), but not necessary for this manuscript.

Response: We thank the reviewer for this thoughtful suggestion. We agree that, while the multipole source simulations, while relevant to future applications, they are not essential to achieving the main objectives of this manuscript. These results were based solely on numerical tests, with no experimental validation. To improve focus and coherence, we have expanded this part of the analysis and moved the detailed results to the Appendix B (lines 710-718).

The main text now includes a concise discussion of the implications of multiple simultaneous impacts, which are crucial for future field deployment. We believe that this approach helps to streamline the main text while ensuring that the results are still available for interested readers.

- **Specific comment 14:** Line 298: fig 7 - Several grammatical errors in the figure caption.

Response: Thank you for pointing this out. We have carefully reviewed and revised the caption of Figure 7, correcting grammatical errors and improving clarity (lines 357-360).

- **Specific comment 15:** Line 328: fig 8 - This caption contains discussion/results content and is unnecessarily long. I would revise it.

Response: Agreed. We have revised the caption to make it more concise and to focus on describing the figure itself (lines 386-390).

- **Specific comment 16:** Lines 338-344: Much of this could be introduced in the methods section, rather than in the results in my opinion.

Response: Thank you for this structural suggestion. The first part of the paragraph in lines 338–344, which describes the experimental setup and definitions, is indeed more appropriate for the Methods section. We have moved this content accordingly, keeping only the actual results in the revised Results section (lines 122-124).

- **Specific comment 17:** Line 408: I believe that figure 11 could be recreated to be more effective. I suggest plotting (a) and (b) by themselves in a vertical orientation, with (c) - (f) in a square orientation separated by a vertical line.

  Response: Agreed. Following your recommendation, we have redesigned the figure. Panels (a) and (b) are now arranged vertically to emphasize the results obtained using the entire signal packet. Panels (c) to (f), which are based on the first wave, are arranged in a 2×2 square layout with a vertical dividing line. We believe that this new layout improves the clarity and interpretability of the figure (line 449).

- **Specific comment 18:** Line 471: Section 4.1.2 -- The discussion of k_IMP is extremely short, given that this is one of two primary functions of this technology. I think this is a critical part of the manuscript that is generally overlooked throughout. This could also be a section where you discuss how this new system might perform under field conditions.

  Response: We appreciate this insightful comment. We agree that the coefficient k_{IMP} (impulses per particle mass) represents a fundamental output of our monitoring system that deserves a more thorough and comprehensive discussion.

  In the revised manuscript, we have expanded Section 4.1.2 to provide a more detailed interpretation of the observed trends in k_{IMP} and to compare these with the results from the SPG system (lines 565-570, lines 571-576). We have also added a discussion on how the PMA system might perform in field conditions in Sect. 4.4. We believe this addition strengthens the manuscript.

- **Specific comment 19:** Line 211-212: Awkward working of this first sentence.

  Response: Thank you for pointing this out. We have revised the sentence for clarity and improved readability (lines 233-234).

- **Specific comment 20:** Line 212: 'firstly' is incorrect

  Response: This sentence was revised (lines 234-235).

- **Specific comment 21:** Line 252: extra parentheses in see eq. 14

  Response: Thank you for spotting this. We have removed the extra parentheses in the revised manuscript.

- **Specific comment 22:** Line 278: Mon. is already introduced as an abbreviation of monopole.

  Response: Thank you. We have removed the redundant definition of “Mon.” in the revised manuscript.

- **Specific comment 23:** Line 305: Mul. is already introduced as an abbreviation of multipole

  Response: Thank you. We have removed the repeated explanation of “Mul.” in the revised manuscript.

Additional clarifications

In addition to the above comments, spelling and grammatical errors pointed out by the reviewer have been corrected in the manuscript.

We look forward to hearing from you in due time regarding our submission and to respond to any further questions and comments you may have.

Sincerely,

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Manuscript ID: egusphere-2024-2525
Original Title: *Improving prediction of particle size with a novel acoustic bedload monitoring system consisting of phased microphone arrays and accelerometer*
Authors: Zheng Chen et al.

Dear Reviewer #2,

We sincerely thank you for your constructive and insightful comments on our manuscript. This feedback has helped us to significantly improve the quality, clarity, and structure of the manuscript. In response to the reviewers' comments, we have thoroughly revised the original manuscript.

Below, we provide a detailed point-by-point response to each comment. All changes in the revised manuscript are highlighted for clarity, and line numbers cited in our responses refer to the revised version of the text.

Responses to the Comments from Reviewer #2

- **General comment 1:** The manuscript by Chen et al. presents a novel approach to river bedload monitoring using a phased acoustic array together with an accelerometer sensor built into a modified version of a Swiss Plate Geophone casing. The study aims to locate bed particles on the cover plate of the device and exploits the wave train content to estimate the particle size. The team presents laboratory aided tests of the principle and performance of the system and compares the results with earlier experiments using the "classic" Swiss Plate Geophone devices.

  The study addresses a relevant and timely topic, suggesting indeed a possible step forward in the field of continuous bedload transport research, going beyond what existing approaches can deliver. The concept of using a phased array appears overall suitable to be enrolled for such a customised case. That said, I feel the study remains somewhat premature and incomplete in the methods and tests it presents and is likewise in a not very organised structure in terms of writing and figure design, which prevents the reader from clearly grasping the line of arguments.

Response: Thank you for your thorough overall assessment of our manuscript. We are encouraged by your recognition that the proposed PMA system represents a significant advancement in the field of continuous bedload transport monitoring and providing improvements on existing impact-based approaches.

We also acknowledge your concerns regarding the immaturity of the methods and the partially unclear structure of the manuscript. In response, we have made substantial revisions to improve the clarity, organization, and presentation throughout the manuscript. Specifically:

We have restructured the Results and Discussion sections to provide a clearer logical flow and better alignment with the experimental design and key outcomes (see revised Sections 3 and 4). The figures and their captions have been revised for clarity, conciseness, and to reduce redundancy with the main text (e.g., Figures 6, 7, and 8); We have improved the language and reduced ambiguity, particularly in the Methods sections, to which we have also added missing details (e.g., sensor specifications, modeling assumptions, and signal processing parameters). In addition, we have added a more forward-looking and integrative Conclusion (Section 5), which more clearly articulates the contributions of this study and how it addresses the core research questions.

We would like to express our sincere appreciation for your critical feedback, which has helped us to significantly improve the clarity and scientific communication of the manuscript.

- **General comment 2:** I see a main unknown and potential flaw in the design to relate impact amplitudes with grain size while keeping the drop height for each particle somewhat constant. It is hard for me to imagine how under natural conditions there will be constant drop heights for particles of varying size. Or, the other way around, how would the approach successfully estimate the correct grain size if river bed particles hit the instrument with different fall velocities and overall differing horizontal velocity vectors? What about those particles that rather slide and roll instead of jumping? These fundamental questions are neither raised nor implicitly addressed by the study, neither in the experiment design nor in the argumentation.
In writing "somewhat constant" in the above paragraph, I wonder why the height of centre of mass of the particles above the plate was kept constant instead of keeping the full vertical drop height constant. This adds a further unnecessary (or unexplained) variable that changes the experiment results.

Response: This is an important comment. We agree that the relationship between impact signal characteristics and particle size is not only influenced by mass, but also by impact velocity and the motion mode (e.g., rolling, sliding, saltation). We would like to clarify and expand on our rationale and methodology regarding this issue.

First, the approach of establishing a correlation between the impact signal amplitude and the particle size is widely used and physically grounded in bedload surrogate sensing. This relationship has been demonstrated by many previous studies, including *Wyss et al*. (2016a) and *Mao et al*. (2018), where larger particles typically generate higher signal amplitudes due to their greater momentum and impact energy. Our results are consistent with this expectation.

Second, we have indeed explored the role of impact velocity in our experiments. Although impact velocity does affect the signal amplitude, our laboratory tests show that its influence is less significant than that of particle size. Similar conclusions were reported by *Wyss et al*. (2016a, WRR), who demonstrated that increases in flow velocity (and thus increasing particle impact velocity) led to only modest increases in signal amplitude of the SPG system across particle sizes (see Figure 6 from *Wyss et al*. (2016a), attached below for reference).

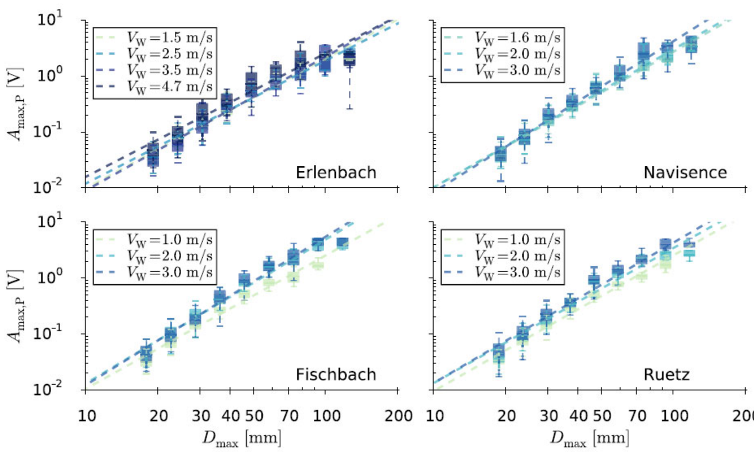

Figure 6 in Wyss et al. (2016a). Maximum amplitude of a packet $A_{max,p}$ as a function of mean particle-size D_m, for different mean flow velocities V_w

In our experiments, we controlled the **drop height based on the particle's center of mass**, resulting in slightly different release heights for particles of different sizes. This design choice was not intended to mimic any specific natural condition, but rather to ensure consistency with previous laboratory calibration studies of SPG systems (*Chen et al*., 2022, JHR), allowing meaningful comparisons to be made across technologies. Moreover, under a fixed center-of-mass drop height, smaller particles do indeed reach slightly higher impact velocities than larger particles. This matches the common trend observed in natural settings whereby smaller bedload particles tend to move faster than larger ones under the same flow and bedload slope conditions. We have added a note to the manuscript to clarify this point.

We agree that the variability in natural particle motion—sliding, rolling, or bouncing—adds further complexity. This study focuses on vertical impacts under controlled conditions, which is a necessary first step towards system calibration. In our earlier research (*Chen et al*., 2022), we explored signal characteristics under different transport modes. We demonstrated that saltation and sliding events can be distinguished using acoustic parameters, such as packet duration and frequency content. These insights will be incorporated into future system calibrations under flume and field conditions.

Finally, the main objective of this study was not to reconstruct every aspect of natural motion, but rather to address a **key source of uncertainty in impact-based systems**: the variability introduced by eccentric impacts on the system plate. Our results show that shifting from centric to eccentric impact positions can reduce signal amplitude by approximately 40% for a given particle size, significantly affecting calibration reliability. The proposed phased microphone array (PMA) system directly addresses this issue by enabling source localization and paving the way for the correction such spatial effects in future field deployments. A more detailed discussion of particle size estimation is included in the updated Sect. 4.4, "Improved estimation of particle size and outlook to field application"

We have revised the manuscript accordingly to better explain the points just mentioned (lines 113-118).

- **General comment 3:** Another, major flaw arises from the inability to locate and quantify the effects of multiple particles being in contact with the plate. As nice as it is to isolate single impacts, under natural conditions the norm will be to experience multiple impacts during a bedload transport period, even during the time window for which the first pulse of an impact signal is collected. If it is impossible to reliably locate two or more sources at a time, it will also be impossible to extract reliable information on the particle size and hence the envisioned total bedload flux across the sensor plate. I might have missed some detail in reading the manuscript but from the results section (e.g. Fig. 7) is seems that the two synchronous impacts cannot be deciphered, as one would expect from the capabilities of a phased array setup, and as is discussed in the manuscript further down.

Response: We thank the reviewer for highlighting this crucial aspect of real-world application—namely, the occurrence of multiple simultaneous or near-simultaneous particle impacts. We agree that resolving multiple acoustic sources is essential for accurately quantifying bedload transport, we would therefore like to clarify the capabilities and current limitations of the proposed PMA system in this regard.

As described in Sections 2.4.3 and 3.1.3 of the manuscript, the PMA system is indeed capable of identifying and localizing multiple sound sources that are activated simultaneously.

This is one of the fundamental advantages of beamforming-based phased array processing. However, spatial resolution limits do apply. Specifically, when two impacts occur too close to each other (i.e., within a few centimeters), the resulting acoustic source image will show a merged peak, making it difficult to distinguish the impacts as separate events. This resolution threshold has been quantified through numerical simulations: under our current sensor layout and signal frequency band, the spatial resolution is approximately 12 cm.

Importantly, this spatial resolution is not fixed. It can be improved by increasing the number of microphone elements, optimizing array geometry, or extending the signal bandwidth. Of course, such improvements entail trade-offs in terms of system cost, complexity, and computational load. These factors will be taken into account in our future design iterations. In response to your comment (as well as specific comments 17, 18 and 20), we have conducted additional numerical tests involving three and four simultaneous sources in different spatial arrangements. These results are now presented in the Appendix B, and their implications are discussed in Sect. 4.3.

We acknowledge the reviewer's concern regarding the possible occurrence of *multiple simultaneous impacts under natural conditions*. Previous work by *Wyss et al*. (2016, JHE) quantified the probability of overlapping impact signals using the time ratio z_p. Their analysis (Fig. 12, also presented below for reference) showed that for transport rates q_s smaller than ~5 kg/s/m, z_p values are typically below 1, indicating that simultaneous impacts are unlikely and single-particle signals can be reliably isolated. Only for intensive transport rates, such as those exceeding ~100 kg/s/m reported for the Erlenbach site (*Hegg and Rickenmann*, 1999), the value of z_p is expected to be close to 1, in which case the number of identified packets from the geophone signal may not deliver accurate measurements.

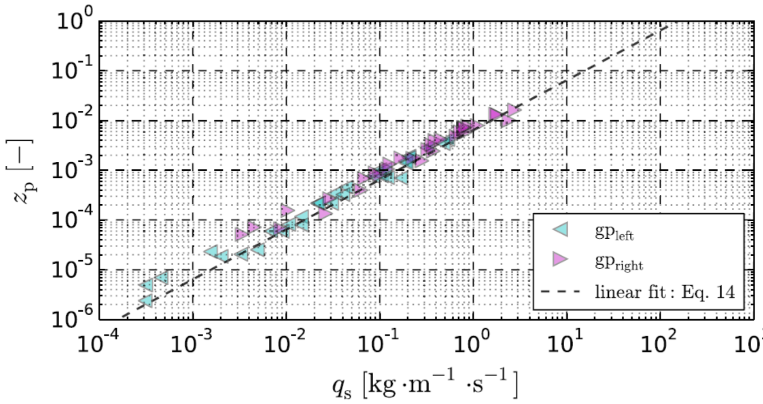

Figure 12 in *Wyss et al*., (2016, JHE). z_p ratio of the left and the right central geophone plates (gp_{left} and gp_{right}) as a function of unit bed load transport rate

We also recognize that real-world bedload transport involves not only discrete point impacts but also tangential sliding and rolling motions that produce spatially extended or moving acoustic sources. While the current study focuses on static or instantaneous point sources, we are actively exploring methods to detect and characterize dynamic source patterns (e.g., migrating impact signatures), which may enable differentiation between saltation, rolling, and sliding events. This will be a central focus of our upcoming flume experiments and algorithm development efforts.

Finally, we would emphasize that the present study is a critical first step: it demonstrates the feasibility of localizing particle impacts with a compact PMA array while also highlighting the importance of spatial impact variability—a known source of uncertainty in previous systems. By enabling localization, the PMA system provides a framework for correcting eccentricity-induced amplitude distortions and eventually integrating spatially distributed impact information under multi-source conditions.

We have revised the manuscript in accordance with the above points to provide a clearer explanation of the capabilities, limitations, and future research directions related to multi-source detection.

- **General comment 4:** The text contains numerous grammatical glitches, equivocal wording occasions, unclear terminology, and cluttered graphics. I list some of those issues in my detailed comments but think the manuscript is not yet at a stage where a fine screen of such improvements makes sense. I suggest to pay due attention to improvements of the presentation quality of the study both in terms of writing and graphical art.
There are several occasions when the text mixes up theory, methods, results and/or discussion and hence requires reorganisation to be consistent. See details below for examples.
Many figures are awkward to read/interpret. Especially the insets of the reference data (Wyss et al.) make any comparison virtually impossible. Likewise, the semantics of the figures not always add up, which is when I would suggest to split figures to maintain consistent pieces of information rather than collections of fragmented information. See details for examples.

Response: We thank the reviewer for detailed comments regarding the quality of overall presentation, including language, structure, and design of figures. We acknowledge that clear communication is essential to ensure the accessibility and reproducibility of our work. We agree that the manuscript requires significant improvement in this regard.

In response, we have carefully revised the entire manuscript to enhance its clarity, consistency, and scientific organization. Specific improvements include:

- Rewriting key paragraphs to ensure that background information, methods and results are not mixed within the same sections.;
- Clarifying terminology and improving sentence structure throughout the text;
- Systematically reworking several figures to improve layout and label readability;
- Splitting or restructuring multi-part figures where necessary to ensure consistency of information per figure.

We also plan to conduct a final round of professional language editing after the manuscript has been revised, in order to further refine it prior to resubmission. We hope that these efforts will substantially improve the quality and clarity of the work.

- **Specific comment 1:** Detailed comments
l. 90-92, in the sensor description section it would help to give more detail on the device characteristics apart from the manufacturer and type ID, e.g. frequency range, sensitivity, digitizer sampling depth, and so on.

Response: Thank you for this helpful suggestion. In response, we have revised the sensor description section (original lines 89–103) to include key technical details. These include the frequency response range and sensitivity of both the microphone and the accelerometer, as well as the sampling rate and bit resolution of the digitizer. These specifications are now clearly stated in the revised manuscript to support better interpretation of the sensor signals and system performance (lines

89-92, lines 99-105).

- **Specific comment 2:** l. 108, 2.2 10^1 kg means 22 kg. Check values

Response: Thank you for pointing out this error. You are correct - the value should be 2.2 kg, not 2.2×10^1 kg. We have corrected this mistake in the revised manuscript (line 111).

- **Specific comment 3:** Fig. 2, why are there so few validation drop locations and overall, why were not more locations used further towards the margin of the plate? It seem quite unlikely that only the central 2/3 of the plate would be impacted by moving bed particles, and the edge effects appear to be quite significant according to the study results.

Response: Thank you for this comment. We agree that, under natural bedload transport conditions, particle impacts may occur over the full plate area, including marginal regions. In this study, we designed a total of **16 impact positions** arranged in a 4×4 matrix across the PMA plate surface (see Fig. 2b), with about 900 individual impact experiments conducted using seven particle size classes. For each test, high-speed video (captured at thousands of frames per second) was used to record the entire impact process. Impact velocity and particle trajectories were extracted frame-by-frame using image analysis algorithms, with key frames—particularly those before and after contact—manually checked to ensure accuracy. Due to the extremely high frame rate and data volume, this workflow is computationally and manually intensive. Therefore, the current experimental design represents a practical compromise between spatial resolution, statistical reliability, and data-processing feasibility.

To address spatial signal variability, we intentionally selected impact positions that cover a wide range of eccentricity distances (0–233 mm). These positions included both centric and highly eccentric locations, all of which were well within the PMA system's effective impact area. The validation positions were also chosen to span both moderate and larger offsets, ensuring that the proposed correction method for eccentricity-induced amplitude bias could be reliably tested.

We acknowledge that further expansion of the test locations—especially towards the extreme edges of the plate—could help refine the calibration model under more extreme boundary conditions. However, as discussed in Sect. 4.4 of the revised text (original Sect. 4.1) and as illustrated in Fig. 11 and Fig. 12, our proposed model has already demonstrated effective performance in reconstructing sound source positions and in correcting for eccentricity effects across the designed test domain. Importantly, Fig. 13 shows that using the inversion-derived source coordinates leads to a significant improvement in particle size estimation, even for highly eccentric positions.

In future work, we plan to explore two complementary strategies to further address edge-effect challenges: (i) **adding more edge-positioned impact points** through targeted testing, and (ii) using **finite element simulations** to extend the spatial response analysis to untested positions beyond the current experimental layout. This will allow us to better characterize the dynamic behavior at the boundaries without greatly increasing experimental effort.

We have added a clarification in the revised manuscript (Sect. 4.4) to reflect these points and acknowledge the edge-effect implications more explicitly (lines 646-655).

- **Specific** The derivation/description of the beam forming technique seems to be or will at least be based on

comment 4: numerous references representing work of generations of scientists. Yet, there is only a single reference included. I suggest the authors give proper credit to the scientists that have built the physical concept, mathematical framework and perhaps computational implementation of the routine – unless all this has indeed been developed by the authors of this study.

Response: We appreciate your reminder. In the revised manuscript, we have added several key references that reflect the development of its physical principles, mathematical formulations, and computational implementation (lines 138-140). These include foundational works in array processing theory (e.g., Merino-Martínez et al., 2019; Brandstein and Ward, 2001), its application in acoustic source localization (e.g., Brooks and Humphreys, 2006; Schmidt, 1982), and practical implementations relevant to geophysical monitoring.

- **Specific comment 5:** Fig. 3 d, R_s needs to be defined

Response: We agree that the definition of R_s should be made more accessible to readers when interpreting Fig. 3d. Although it is defined in the main text (original lines 193–195), we have now added a brief explanation to the figure caption for clarity (lines 179-180).

We have also ensured consistency in the choice of resolution threshold (30% of the peak value) across all relevant figures. However, Fig. 12 presents both the 30% and 10% definitions for comparison.

- **Specific comment 6:** l. 169, the heading (simulation) does not depict what the text below refers to (construction details)

Response: The original subsection heading "Numerical simulation procedure" did not fully reflect the content that follows, which includes both model assumptions and setup details. We have revised the heading to better reflect the section's scope, which now reads "Numerical simulation setup" in the revised manuscript (line 182).

- **Specific comment 7:** l. 171, 3D grid does not make sense, either it is a grid (2D) or a 3D structure (voxels, cube). Also, pixels is a grid-world term while nodes is a vector-based term. This (and other occasions) need to be corrected throughout.

Response: Thank you for this clarification. In the revised manuscript, we now refer to a 3D volumetric domain that would require discretization into voxels for full 3D source reconstruction. Since our study adopts a 2D surface scanning approach, we consistently use the term "nodes" to describe the discrete sampling points over the 2D scanning plane.

We have revised the text accordingly to reflect accurate terminology throughout this section (lines 187-192, lines 196-201).

- **Specific comment 8:** l. 184, the equation provides rather trivial information, consider removing it

Response: Agreed. Accordingly, we have removed Eq. (11) and described the grid resolution in the revised manuscript (line 198).

- **Specific comment 9:** l. 194-196, this is more about phased array theory than actual methods employed in this study. Consider moving this to the introduction

Response: We agree that the original paragraph in lines 194–196 included general theoretical information about phased array beam patterns, which may be better suited for background. However, since the definition of array resolution R_S is a key parameter used in our signal evaluation (e.g., in Fig. 5e, Fig. 6e, and Fig. 12), we have retained the definition in the methods section but have revised the paragraph to be more concise and focused on the specific implementation adopted in this study (lines 210-212).

- **Specific comment 10:** l. 199-204, there are some references needed to support the statements in this section

Response: Agreed. In the revised manuscript, we have added relevant citations to foundational works in array signal processing (e.g., Flanagan, 1985; Brandstein and Ward, 2001) that cover the effects of sensor spacing and frequency on beam pattern sharpness and aliasing. These citations provide theoretical support for the design considerations described in this section (line 219).

- **Specific comment 11:** l. 600, "allowing the spatial location", yes but only when a single impact is isolated, which I think will be of little help under real world conditions.

Response: Thank you for this comment. To avoid overstatement, we have revised the sentence in the revised manuscript (lines 691-692).

Specifically, we now clarify that spatial localization is feasible primarily under isolated or sparsely distributed impact conditions. We also refer to our resolution analysis, which quantifies the system's spatial resolution (~12 cm) for closely spaced sources under the current configuration (lines 693-694).

- **Specific comment 12:** l. 204-209, there is some rather trivial information that might be better placed in a general introduction (or just skipped) rather than being an actual topic of "Microphone elements arrangement".

Response: We thank the reviewer for the suggestion. The paragraph in question was originally intended to explain the rationale behind the design of the microphone spacing in the PMA system during numerical simulations, based on the Nyquist sampling criterion. Specifically, we assumed a representative signal frequency of 1600 Hz for simulation purposes, which corresponds to the characteristic frequency observed in the experimental results for the tested particle size class. Based on this frequency, the upper limit for microphone spacing was calculated. This design parameter is directly relevant to the beamforming performance of the system and thus was included in the section on microphone array configuration.

To improve clarity and avoid unnecessary detail, we have now revised the paragraph to streamline the description. Additionally, we retained the reference to Table 2 in the methods section, where the simulation parameters (including angular frequency and wave number) are summarized and first introduced in the context of the numerical beamforming model.

We hope this revised presentation better contextualizes the design rationale without overloading the section with technical derivations (lines 223-231).

- **Specific comment 13:** l. 213, why a sine wave and not the more likely to expect pulse signal? Can you explain/justify this? Will this decision not affect the validity of the test outputs with respect to the envisioned natural settings?

Response: We appreciate the reviewer's thoughtful question regarding the choice of input waveform in the numerical tests. The use of a periodic cosine (or sine) wave was a deliberate decision based on both practical and physical considerations.

First, from a modeling standpoint, a periodic sine wave offers precise control over the signal frequency, which is crucial for evaluating the performance of the beamforming algorithm under varying array configurations (e.g., sensor spacing, number of elements, and multi-source scenarios). This allows us to isolate the influence of array design on spatial localization accuracy in a controlled manner, independently of complex signal shapes or broadband spectra.

Second, the sine waveform is also physically justifiable. Impact-induced acoustic signals recorded in our laboratory experiments often exhibit an initial phase of high-frequency oscillation that closely resembles a few cycles of waveform. As shown in Fig. 4d, the impact-induced signals recorded by a microphone element in our laboratory display an initial phase of rapid oscillation lasting 1–3 ms. These oscillations result from the short-duration contact between the particle and plate and are affected by particle size, contact stiffness, and impact dynamics. Similar behaviors have been reported in SPG system data and in finite element simulations (*Chen et al*., 2022 https://doi.org/10.1080/00221686.2022.2059585), supporting the physical relevance of our waveform choice.

Furthermore, the half-sine pulse is widely recognized as a classical form of mechanical shock loading in standards such as IEC 60068-2-27. Our use of a short-duration periodic cosine wave can be seen as an idealized extension of this fundamental pulse type for the purpose of numerical analysis.

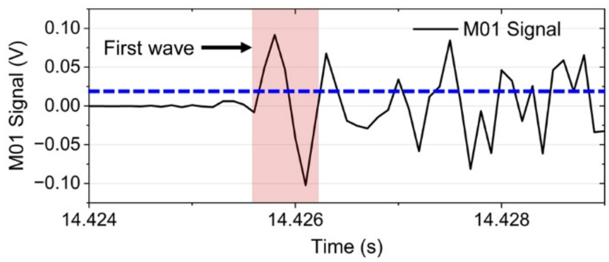

Figure 4(d) in the main text. Illustration of the first wave of the microphone signal.

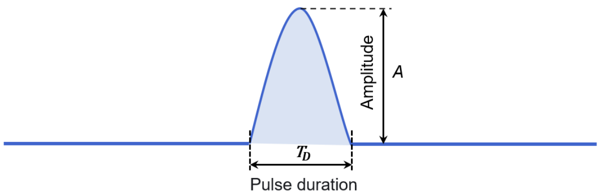

Figure: The half-sine pulse

We agree that an impulse-type input could also be used in such simulations, and it may better reflect certain broadband aspects of real-world signals. However, we believe this difference in source waveform has limited impact on the key findings of this study, which primarily focus on the system's ability to localize and reconstruct acoustic sources using beamforming principles. Most importantly, the PMA system is not solely evaluated based on simulations—its performance is being validated through extensive physical tests, including controlled drop experiments and future flume and field applications, which will incorporate the full complexity of natural impact signals.

We have added clarification to this point in the revised manuscript (see Sect. 2.4.3, lines

234–237).

- **Specific comment 14:** l. 214, why adding white noise? Can you justify, both why adding noise first of all and then also why specifically white noise? I would rather expect to see red noise or some derivative of it to better reflect "real" conditions.

  Response: Thank you for the insightful comment. In the numerical tests, we added white Gaussian noise to the synthetic signals received by each microphone element to simulate moderate noise contamination and evaluate the robustness of the beamforming algorithm under non-ideal conditions. The choice of white noise with an SNR of 10 follows a common practice in signal processing and seismological inversion studies, where white noise is used to test algorithmic stability and sensitivity due to its flat spectral content.

  We agree that actual ambient noise in fluvial or field environments may be better represented by colored noise such as red or pink noise, which typically exhibits stronger low-frequency components. However, the goal of this test was not to simulate field noise conditions, but rather to introduce a controlled perturbation to assess spatial resolution under noise. Future simulations may incorporate more realistic noise structures as we expand the modeling to more complex field scenarios.

  We have added statements in the revised manuscript (lines 239-241).

- **Specific comment 15:** l. 224, replace "centric" by "central"
  l. 224, why indexing a repeat experiment run?

  Response: Agreed. We have replaced the term "centric" with "central" in this sentence to follow standard technical usage. Regarding the indexing of the microphone and impact location (e.g., M05 and 1.4), these were intended to refer to the sensor ID and the specific impact location in the impact matrix shown in Fig. 2b. We have clarified the wording to ensure this is more intuitive and removed unnecessary indexing from the sentence where appropriate (line 251).

- **Specific comment 16:** Fig. 6, it would help quite a bit if the panels a-d would indicate the locations of the sensors on the coloured matrices, instead of having to look for Appendix A to get that information.

  Response: Agreed. We appreciate that both you and Reviewer #1 identified the need for clearer visualization of sensor locations in Figure 6. In response, we have revised Figure 6a–d to directly overlay the microphone element positions on the acoustic source maps. This change eliminates the need to refer to Appendix A when interpreting the different array configurations and significantly improves figure readability.

- **Specific comment 17:** Fig. 7, better split the figure into single impacts and multiple impacts. In addition, in (a) and (b) there are only two locations shown, in (e) there are three locations shown. Similar for c,d and f. Please resolve.

  Response: Agreed. In the revised manuscript, we have addressed this in two ways. First, we split the original Figure 7 into two separate figures: one for single-source scenarios and one for multi-source scenarios. This improves visual clarity and helps distinguish the objectives of each test. Second, we updated the subplots and captions to clearly indicate the number and spatial arrangement of the impact locations in each case.

  We have conducted new simulations involving three and four simultaneous sources, which are now presented in the Appendix B. These additions further illustrate how the PMA system performs under more complex multi-source conditions. We believe these revisions enhance the clarity and completeness of the analysis.

- **Specific comment 18:** l. 309-310, I disagree with the statement that the locations agree well. This is just an artefact not a real two-source location. Actually we need proper tests here to show the real artefacts arising from the case of more than one impacting particle being relocated. What if two, three, four, … particles impact at the same time? What is the effect of different locations (up-down, oblique, different distances from center)? Here I only see two sources horizontally symmetrically impacting the plate.

Response: Thank you for this critical feedback. We agree that the original two-source simulation presented a highly simplified and symmetric case, which does not fully represent the complexity of real-world multi-particle impact scenarios. In response to your suggestion, we have conducted additional numerical simulations involving three and four sources at varying spatial locations, including non-symmetric and off-center configurations. These results help to better illustrate the performance and limitations of the PMA beamforming algorithm under more realistic source interference conditions.

To keep the main manuscript focused, we have moved all multi-source simulation results to the Appendix B and expanded the corresponding discussion in the main text (Section 4.3) to reflect the key insights. These new tests demonstrate that the current system can distinguish multiple sources, but also highlight challenges in resolving closely spaced or overlapping sources. We thank you for encouraging this important clarification.

- **Specific comment 19:** l. 316, "indicating that the grain of the noises become weaker…", this sentence does not make any sense to me

Response: Thank you for pointing out this unclear sentence. We have rephrased it to express the statement more clearly in the revised manuscript (lines 354-355).

- **Specific comment 20:** l. 324-326, this is interpretation and should not occur in the results section. Albeit, what have we learnt from that exercise after all? We already know that a phased array cannot locate two synchronous signals from two different locations. Also, what does that imply (later in the discussion) for a real world case when multiple impacts happen?

Response: Thank you for raising this important point. In the revised manuscript, we have rephrased the original sentence to describe the observed beamforming output in neutral, data-driven terms, and we have moved the interpretation regarding spatial resolution limits to the Discussion section (Sect. 4.3).

Regarding the broader implication of this test, we acknowledge that the symmetric two-source configuration originally presented is simplified and does not fully represent the complexity of real-world bedload transport, where multiple impacts may occur simultaneously at varying distances and directions. In response to your comment (as well as general comment 3 and specific comment 18), we have conducted additional numerical tests involving three and four sources in non-symmetric, more realistic spatial arrangements. These results are now presented in the Appendix B, and their implications are discussed in Sect. 4.3.

While it is true that classical phased array beamforming cannot perfectly resolve multiple closely spaced sources, our simulations provide a first quantification of the spatial resolution limit (~12 cm) under the current system configuration. This information is crucial for designing future sensor layouts and for understanding the trade-offs involved in multi-source detection. We also note that this threshold is not fixed—it can be improved by using more microphone elements, developing denser layouts, and including broader frequency content. We plan to explore these possibilities in upcoming system iterations and flume experiments.

- **Specific comment 21:** Fig. 8, any comparison with the inset data is not possible (also in figs. 9 and 10), please add those data points to the main chart, perhaps with clearly distinct colours. Perhaps add a small map that depicts the impact locations to prevent readers from searching back and forth to get the context right. Add the number of samples that are summarised in the box plots. Perhaps remove the x-axis line to make clear this is categorical data and not continuous, because at a first glance one could think the four box plots per diameter are a continuum of sizes. In summery, just make the job easier for readers to correctly interpret the figure.

Response: Thank you for your thoughtful suggestions regarding the clarity and interpretability of Figures 8–10. We have substantially revised Figures 8 and 9 based on your comments. Specifically, in Figures 8a and 9a, we replaced the previous boxplots with mean values and error bars (standard deviation) from repeated impact tests. These data are now plotted directly in log-log coordinates, making them easier to compare with previously published flume and field data (e.g., from *Wyss et al.*, 2016a and 2016c). This approach addresses your concern about inset readability by eliminating the need for a separate inset panel.

In addition, we added a small schematic within Figure 8b to illustrate the four impact locations used in the tests. This helps readers avoid having to refer back to earlier figures to understand the spatial context. Similar improvements have also been applied to Figures 9 and 10.

We hope these improvements enhance the readability and interpretability of these figures.

- **Specific comment 22:** l. 417-418, this should go to the methods section. In addition, why not just calculating the RMSE between real location and model derived location?
l. 422, "suggesting a good agreement", first this is interpretation and second it is a weak statement that can and should be quantified.

Response: Thank you for the constructive comment. We have moved the relevant description to the Methods section as suggested. In addition to the previously reported dimensionless accuracy ratio $r_j^{m,t}$, we have now computed the root mean square error (RMSE) between the estimated and true impact coordinates across test locations. The average RMSE was approximate 4 cm in the X direction and 3 cm in the Y direction, indicating good absolute localization accuracy under our current sensor configuration. These updates have been reflected in the revised manuscript (lines 465-469).

It is also worth noting that this single-source localization accuracy (quantified via RMSE and dimensionless offset ratio) is conceptually distinct from the *spatial resolution* of the PMA system, that is, the minimum separation distance at which two simultaneous sources can be resolved. As shown in our multi-source simulations, this resolution is approximately 12 cm under the current sensor layout and signal conditions. Together, these metrics provide a more complete evaluation of the PMA system's localization capability.

- **Specific comment 23:** l. 428, "boxes in green", I see no green boxes at all

  Response: Thank you for pointing this out. We have corrected this in the revised manuscript to "boxes in blue" which now accurately reflects the coloring used in the figure (line 473).

- **Specific comment 24:** l. 429, what are these values (30 % and 10 %) based on? Are these arbitrarily chosen or based on some reference or some physical meaning?

  Response: The 30% and 10% thresholds used to characterize the beamwidth were chosen as empirical reference levels to complement the standard 3 dB definition. These values do not correspond to specific physical thresholds but were selected to explore the effect of stricter amplitude cutoffs on spatial localization estimates. The idea was to evaluate how the spatial extent of the main lobe changes under different amplitude criteria, which can be useful when assessing localization sensitivity or when noise levels vary.

  We have clarified this in the revised manuscript (lines 210-214).

- **Specific comment 25:** l. 445-446, this sentence needs some context in which is shall be embedded. Currently, it hangs in the air with no logic

  Response: Agreed. We have restructured the paragraph to provide a smoother transition, which improves the logical flow of the discussion (lines 523-525, lines 530-533).

- **Specific comment 26:** l. 466-470, but this is neither shown nor is this discussed in terms of its consequences for the ultimate goal, to get a reliable bedload flux estimate

  Response: Thank you for highlighting this critical issue. We agree that the original paragraph introduced the idea of using acoustic source localization to compensate for amplitude variability, but did not fully elaborate on how this step contributes to improved bedload particle size or flux estimation.

  We have clarified this in the revised manuscript conceptual link in Section 4.4 (Improved estimation of particle size and outlook to field application). Specifically, we now explain that one of the major sources of uncertainty in amplitude-based grain size estimation is the spatial variability of impact locations. The PMA system's ability to localize these positions allows for the construction of a spatial compensation function that can correct for this variability.

  To improve the overall logical flow, we have also reorganized the structure of the Discussion section. The revised order progresses from system performance and impact location effects to spatial resolution, and finally to the improved estimation of particle size.

- **Specific comment 27:** l. 476-485, the discussion is weak here, it needs to go for more far reaching thoughts, beyond just interpreting possible reasons why this system deviates from earlier/other SPG devices. The title of the study tells us the new system improves the prediction of particle size for bedload measurements. Yet, this is not clearly presented.

  Response: Thank you for your comment. To clarify, the manuscript already includes a dedicated section titled *4.4 Improved estimation of particle size and outlook to field application* (previously Sect. 4.2), which presents our approach and findings in this regard.

- **Specific comment 28:** l. 505-510, this part is fairly repetitive, it reiterates the results described earlier but lacks an interpretation

Response: Thank you for this comment. In the revised version, we have streamlined the text to avoid redundancy and added interpretation to clarify the significance of the findings. Specifically, we emphasize that the relative stability of the exponent β across different impact locations suggests it can be treated as a location-independent parameter, whereas variations in α indicate the need for spatial compensation when estimating particle size from signal amplitude (Sect. 4.4).

- **Specific comment 29:** l. 513-536, this reads like methods description, certainly not discussion of results.

Response: Thank you for the comment. In the revised manuscript, we have relocated this content to a new Methods subsection (Sect. 2.6, "Particle size estimation and model-based correction"), where we outline the estimation procedure, correction workflow, and equations used. The Discussion now focuses on interpreting the implications of the results rather than restating the procedures.

A new results subsection titled "Optimized particle size estimation" has been added (Sect. 3.4) to present the quantitative outcomes of the correction method, including comparisons between optimized and uncorrected estimates.

- **Specific comment 30:** l. 534-538, this does not make sense. Ignoring friction any particle that falls from the same height would have the same impact velocity. This may leap back to my very above comment of using the centre of mass to define the fall height instead of the lower margin of the impactor.

Response: Thank you for the comment. We acknowledge that under idealized conditions (ignoring friction and air resistance), any particle dropped from the same center-of-mass height would indeed attain the same impact velocity, regardless of size. In our experimental design, we deliberately controlled for this by defining the drop height from the center of mass of each sphere, not from its base. This approach ensures consistency across different particle size classes and follows established protocols used in previous SPG calibration experiments, thereby facilitating comparative interpretation.

As you correctly noted, this issue overlaps with your earlier Comment 2, which we have also addressed. To avoid redundancy and improve clarity, the original statement in lines 534–538 has been removed from the revised manuscript. We thank the reviewer again for pointing out this issue.

- **Specific comment 31:** l. 540-546, this is a weak interpretation approach, with many "may" and "might" terms but with very little rational arguments that would support the hypothesis. Either add experimental, reference or logical support or skip the attempt of interpretation.

Response: Thank you for your valuable feedback. We have rewritten this section to present a clearer and more logically supported interpretation in the revised manuscript (Sect. 4.4, lines 646-655).

- **Specific comment 32:** l. 558-560, this is repetitive
l. 564-566, this is repetitive

Response: Thank you for these comments. In the revised manuscript, we have consolidated these points into more concise summary sentences that avoid redundancy while highlighting the key outcome. This revision improves the clarity the section (Sect. 4.3, lines 607 and 611-613).

- **Specific** l. 574, "the decrease of R_s tends to weaken", what does that mean? I do not understand.

comment 33:

Response: We have revised the wording to clarify that the reduction in R_s becomes progressively smaller as the number of microphone elements increases, indicating a gradual saturation of spatial resolution improvement at higher array densities. This change has been implemented in the revised manuscript to improve clarity (lines 503-505 in Sect. 4.1).

- **Specific comment 34:** l. 582, "developed" would be quite a bit an overstatement, better use "presented"

Response: Agreed. we have restructured the conclusion section in the revised manuscript (Sect. 5).

- **Specific comment 35:** The fundamental parts of the conclusion chapter are repetitive, giving a mere summary of details described above. A proper conclusion should step beyond the above presented material and ask "what do we know now, or what can we do now?" It shall feed back to the main research question and answer it.

Response: Agreed. In the revised manuscript, we have restructured the conclusion section to explicitly revisit the main research question, summarize the key findings in terms of their scientific and practical value, and highlight the methodological advancements made. We also added a forward-looking paragraph outlining future applications and research directions. We believe this revised conclusion better meets the expectations of a comprehensive and insightful closing section (lines 677-701 in Sect. 5).

Additional clarifications

In addition to the above comments, spelling and grammatical errors have been corrected in the manuscript. We look forward to hearing from you in due time regarding our submission and to respond to any further questions and comments you may have.

Sincerely,

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